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Art. I. *On the Desquamation of certain Rocks, and on its Connexion with the concretionary Structure.* By J. Mac Culloch, M.D., F.R.S.

It is well known that many rocks of the trap family undergo a process of desquamation after a long exposure to air, and are thus gradually resolved into crusts which continue to fall off in succession, at length mouldering into clay. The same appearance, although more rarely, occurs also in granite; and in both cases, it has been conceived to depend on an internal concretionary structure, and to indicate the mode in which the constituent parts of the rock are arranged, which, however invisible in the fresh fracture, is thus rendered evident by the progress of decomposition. In the case of the columnar traps, whether basalt or greenstone, this desquamation often proceeds in such a manner from the circumference of a joint towards the centre, that the ultimate result is a spheroidal body.

This effect, compared with that spheroidal concretionary structure which is known to take place in basalt artificially fused, has appeared sufficient to justify the general conclusion, that all appearances of a similar nature depend on the same cause; and, by a slight addition, it has been held sufficient to account for the jointed and columnar structure of the rock in which it occurs. Having met with some remarkable instances in which this effect was decidedly independent of any concretionary structure, I was induced to examine more particularly the nature of the evidence which had been held sufficient to prove

the current explanation; lest, having been proved inadequate to the solution of many cases of this nature, it might turn out purely hypothetical and inapplicable to any. The result has been, that two causes, perfectly distinct from each other, operate in producing the same effect; and it will be useful to state the facts in question, not only as a caution against the universal adoption of a well known rule in philosophizing, and against a hasty mode of generalization, too often adopted in geological science, but as forming an interesting circumstance in the history of the unstratified rocks, trap, and granite.

I was first led to suspect the truth of the current opinion respecting the dependence of the desquamation of rocks on an internal concretionary structure, by casually examining the columns of granite lately brought from Leptis in Africa, and now lying in the great court of the British Museum. I was surprised to find that the shafts of these columns were in the act of desquamation, casting off crusts precisely similar to those which occur in many cases in natural blocks of granite, and equally resembling, except in their superior integrity, those that are found on the surfaces of the columnar trap rocks after exposure to the weather. As it was obvious that the forms of the shafts could bear no relation to the original form of the blocks of granite, from which they had been wrought, it necessarily followed, that in this case at least, the desquamation could not depend on an internal concretionary structure, but must have resulted from the action of the weather on the exposed surfaces.

These specimens being fortunately accessible to many of our geological readers, they will be enabled to confirm, by their own observation, this important fact; nor can the report undergo the hazard of being suspected of inaccuracy or mis-statements; an expedient to which recourse is often had for the purpose of obviating a difficulty, or eluding a fact which militates against some favourite hypothesis, when such a fact is distant or difficult of access; a proceeding which is indeed unfortunately but too much justified by the numerous inaccurate statements in which geological writings abound.

It is worthy of remark, that, in this case, the detached crust is not decomposed, and that, except in tenderness and fragility, it appears scarcely changed from its natural state. Nor is any stratum of clay or decomposed matter found at the place where the crust separates from the solid block; at least none such was found in the places which I had an opportunity of examining. The thickness of the crust appeared to be between the sixth and the eighth of an inch; but it seemed unnecessary to make any accurate measurement of that which is accessible, perhaps to all who may be interested in the subject of this paper.

The decomposition of rocks has, in most cases, and with justice, been attributed to changes in the state of the iron entering into their composition; but it is evident, that neither this, nor the other causes which have been supposed to produce that effect, are capable of explaining the very singular process in question. As it is in vain to speculate on that to which there is no known analogy, I shall not attempt it. It must, nevertheless, be concluded, that this effect cannot arise from that action of the atmosphere which produces the ordinary effect of weathering; nor is it conceivable how it should affect the interior part of the stone, while the surface more immediately exposed to its agency has escaped.

Being in possession of this singular fact, I was induced, as before remarked, to suspect the common opinion respecting the cause of the desquamation of granite, and was therefore led to re-examine with more attention those causes in nature where the same process takes place on the exposed surfaces.

In the greater numbers of these, I only found that which has been observed by every one, namely, that the desquamation of the prismatic or cuboidal masses of granite which are susceptible of this change, took place all round the surface, respecting some imaginary point or centre, and promising, in the progress of time, to reduce the whole to a smaller and more spheroidal mass. Hence, no conclusion could be drawn as to the cause; since the desquamation might in this case be the result of an internal concretionary and laminar structure respecting

one centre, or the consequence of a process similar to that occurring in the columns of Leptis.

But in examining other cases of granitic desquamation, a different appearance was observed. In these it was found, that a single block desquamated in a manner so complicated that no parallelism was maintained between the surfaces of the stone and the crust; and as, in some cases, such blocks were so thoroughly softened as to admit of being cut with a spade, it was easily seen that more than one, or even two centres of desquamation existed in a single mass; the surfaces of the different spheroids interfering with and compressing each other where they came into contact, and the vacuities still required to fill up the solid, consisting of deficient portions of crusts respecting one or other of the approximate imbedded or internal spheroids. It is evident, that in these instances, the effect could not have resulted from that exterior action, be it of whatever nature, which produced the crusts on the columns already described, but must have been determined by other causes depending on an interior spheroidal and concretionary structure; the existence of such a structure being further supported by other well known analogous circumstances in rocks, as well as by those already mentioned which occur in basalt artificially fused, and those which are occasionally found in glass under particular circumstances.

Thus, therefore, it is proved, that granite is susceptible of desquamation from two causes, namely, from an internal concretionary structure of a spheroidal and laminar nature, and in consequence of the exposure of its surfaces to the atmosphere independently of any such structure.

But wherever, in blocks of granite of a cuboidal form, the whole surface desquamates, it has already been shown that it cannot be determined to which of these causes this effect must be assigned; and the same difficulty occurs in those cases where the desquamation takes place in straight laminæ. This is the case of schistose granite, as it has been termed, and it requires a further consideration. As the Island of Arran affords the most accessible examples of schistose granite with which I am acquainted, I shall describe the appearances which it there pre-

sents. Those who may have an opportunity, and who are desirous of examining and verifying this statement, will find it situated in the upper part of Glen Catcol, nearly at the very rise of the stream which flows through that valley.

This granite is of a fine texture, and consists principally of a pale feldspar, intermixed with a moderate proportion of quartz; small crystals of hornblende being sparingly interspersed. It is disposed most generally in extended laminæ of large dimensions, but is also occasionally prismatic; the prisms being however very irregular, and commonly of a small size.

The schistose specimens, or the examples of desquamation, which are here the principal objects of notice, do not occur in all the granite at this place; nor is there any apparent cause for its capricious occurrence, as it is sometimes present, and at others wanting, where the eye does not trace any differences in the appearance or composition of the mass.

It does not often happen that more than one laminæ can be detached from a block; and when removed, no further indication of a similar tendency is in general to be discovered. Neither does the forcible fracture of these blocks detect the slightest indication of a laminar structure; the whole appearing as uniform as any granite which is not marked by this remarkable character. It must also be observed, that there is no disposition in integrant parts of the granite which can account for this desquamation. It does not bear the slightest resemblance even to those specimens of gneiss which approach most nearly to the granite character, nor is there the least indication of a foliated disposition in the integrant minerals; the whole texture being as uniform in the detached laminæ as in the block, and being indeed peculiarly compact.

Although two schists, or laminæ, cannot be detached in succession from one surface, there is sometimes the commencement of a second, indicated by a line on the exterior side, into which the blade of a knife can be insinuated for a small space. But it is more common to find this indication alone; the laminæ previously formed, having fallen off and been washed away

by the rains or streams; producing fragments and gravel, among which imperfect and broken specimens are easily procured. It is now necessary to add, that in these cases, the incipient desquamation cannot be completed by force. Neither, on breaking through the whole block, where the partial division of the laminæ from it terminates, could it be suspected that the process would continue further; as no change whatever is perceptible at the part where, without doubt, it is hereafter to be prolonged.

In examining the detached laminæ, the texture and appearance seem in no way different from those of the solid mass whence it was removed. Nor, as far as can be judged, is it less tenacious than an artificial lamina of the same thickness would be; appearing indeed in every respect perfectly natural. Neither does the surface exhibit any signs of decomposition, being on the contrary brilliant and clean, as if cut by art; even the feldspar scarcely assuming a perceptible degree of that opake whiteness which this mineral so generally puts on after exposure to air. The same cleanness and freshness of both the surfaces in contact, are also found where the laminæ and the block are separated; nor is there any loose matter generated, or any appearances of decomposition in the plane which disjoins them.

With respect to the thickness of the laminæ, it was found to vary from the eighth to the third of an inch, but it most commonly ranged from the sixth to the quarter. Although sometimes of a constant dimension throughout, they are observed to be more frequently variable in thickness; having a greater dimension at one side than another, or the surfaces being inclined. The greatest superficial extent of the laminæ which was observed, did not exceed eighteen or twenty square inches, but, from their brittleness and thickness, they can rarely be procured of half that size.

It now remains to inquire, as already stated, whether this process of desquamation is the result of an internal schistose structure invisible in the solid block, as it is in those cases where it is spheroidal from different centres, and only called into action by exposure to the atmosphere, or whether, as in the case

of the columns of Leptis, it arises from some unknown effect of the weather on the surfaces of rocks possessing no original peculiar structure of this nature.

If we were to form a judgment from merely comparing the appearances in the case of the schistose granite, and in that of the columns, we should be inclined to refer them both to the same mysterious cause. The appearances in both cases are precisely similar, and, in both, they differ from that case in which the desquamation is demonstrated to be the result of an internal concretionary structure; since, in this, it invariably happens that the scales are not only separated, but decomposed, losing their integrity, and falling to powder and gravel on the slightest injury. In this case also, the desquamation and decomposition affect many successive lamina, proceeding in some cases, indeed, even to the central point, or to the heart of the block: whereas, in the case of the schistose granite, as in that of the artificial columns, only one lamina, or a minute space beneath the immediate surface, is affected.

Those who are in haste to establish laws and complete theories, little concerned whether or not they are well founded, will be content to repose in this conclusion. But those, to whom truth has a value superior to all other considerations, will prefer doubt to decision, while any ground for hesitation remains. It has already been stated, that in the columns at the British Museum, the desquamation took place over the whole surface, so as to have produced, could it have been completed all round, a hollow cylinder. The same occurs in the decomposition of those natural blocks where the laminæ refer to one centre only. But in the case of the schistose granite, it must be observed, that the desquamation only took place on one surface, and that surface parallel to the chief planes of the great laminæ in which the granite is disposed. No desquamation at right angles to this was observed; or while the larger planes gave off scales, the narrower lateral ones were free from them. This should not have happened had the effect arisen from the same cause, as in the artificial columns; but every exposed surface should have desquamated alike. On the contrary, the desquamation was

parallel, as already observed, to the great laminar structure; and hence it might be concluded, that both originated in the same cause, or that the great concretionary structure which formed the large lamina, was also the cause which influenced the desquamation of the small one. In this state of doubt must this subject at present remain.

It has, however, been sufficiently proved, that the process of desquamation in granite does arise from two causes; but to which of those two the case of the schistose granite is to be referred, must be a subject for future investigation. The facts are in themselves singular; the most remarkable have never yet been noticed, nor have those which were formerly known been examined with the attention they deserve. They have carelessly been referred to one cause, and the present discussion will have its use, even though it has not cleared up the difficulties of the subject, by turning the attention of geologists to one among many of those more recondite points in the history of rocks, from the study of which geology has already derived, and is probably destined to derive, much light.

I shall now proceed to examine some cases in the rocks of the trap family, which are of a similar nature, and which have been equally neglected; as in these also the appearances in question have invariably been referred to that one cause already shown to be far from universal, namely, a peculiar concretionary structure. It will be seen, that although some of the examples of this occurrence in the trap rocks, are truly dependant on this cause, others are as unquestionably the result of actions similar to those which produce the desquamation in artificial blocks of granite. Thus they confirm the views, already held out, of one common effect proceeding from two causes, while they also offer another analogy to add to the numerous resemblances which exist between granite and the rocks of the trap family

I need not call to the reader's attention the cases in which columnar traps, whether basalt or green-stone, are known to desquamate, as they are innumerable, and as instances of them cannot fail to be known to all those who have any practical acquaintance with the rocks of this family. In these cases, the

crusts fall off in succession, in such a manner that the angular or prismatic form at length disappears; and the ultimate result is therefore a spheroidal body, destined ultimately to be also resolved into clay. It most frequently happens, that as the crusts are separated they also fall to pieces, or become loose clay; but occasionally they retain a considerable degree of tenacity, although from the changes of their colour from the natural dark blue to brown, it is evident that the iron has undergone a chemical alteration, being converted from protoxide to rust.

Where similar rocks possess a rude and imperfect prismatic structure, the same effects also take place; the surfaces of the prisms desquamating in such a manner as to leave a congeries of spheroidal or ellipsoidal bodies, destined, in the same way, to be ultimately resolved into loose earth.

But these changes are not limited to prismatic trap only, since they occur in other and different forms; being attended with the same final result, whatever the shape of the block may have been, namely, the ultimate production of spheroids. In some rare instances of this nature, it is remarkable that a loose spheroid is sometimes found, on fracture, to be imbedded in an irregular mass apparently solid, or at least giving no exterior signs of desquamation, and bearing no other marks of change than the loss of the blue and the assumption of the brown colour.

Although, in some of these instances, it might be imagined that the effect of desquamation was produced, as in the case of the artificial granite columns, by the exposure of the surfaces to the air, it will, I believe, be found that, in nearly all, they truly depend on an internal concretionary structure. In the case of aggregated columns, for example, the contact of the prism is generally so complete that neither water nor air can gain access to the surfaces.

The same rule sometimes holds with respect to the joints; although, in many of these, perhaps in most, the contact is not so perfect as to exclude either air or water, as is proved by the changes of colour which their surfaces exhibit. But the chief

argument for an internal structure as the cause of the desquamation of prisms, will be found to consist in the effects which take place in those that are not joined, as well as in those masses of trap which affect a prismatic fracture without being absolutely divided into prismatic forms.

In these it may be observed, as in some of the cases of exfoliating granite formerly cited, that the desquamation is of a complicated nature, referring to more than one centre. Thus, in a single unjointed prism, the same result takes place as in those with joints; numerous spheroids being formed in its length by the process of desquamation. The same effect is produced in those irregular masses that are merely characterized by a prismatic fracture, as the exfoliation commences in many different places, referable to different points, so as to leave, in the same way, a number of spheroidal bodies imbedded in a mass of loose crusts and clay.

Indeed it is further often to be observed, that, in cuboidal or otherwise irregular blocks which have neither prismatic form nor tendency, there are several centres of exfoliation; numerous balls being then finally extricated from a single solid block, of which all the surfaces are equally exposed to the action or contact of the atmosphere.

It appears therefore unquestionable, that in all the instances now enumerated, where the exfoliations respect one or more centres, and the detached crusts are therefore of spheroidal forms, the original cause consists in a spheroidal concretionary structure in the rock, of a nature more or less complicated, although we are equally far from being able to explain in what manner the action of the elements operates on that structure so as to produce the peculiar effects in question. In terminating this part of the subject, I shall quote an analogy in favour of the existence of this structure in prismatic traps, which being, in other respects, of an interesting nature, I purpose to give a more full account of it at some future period.

In this case, which is found in an argillaceous sand-stone, a large bed of that rock is in some places not only marked by a tendency to prismatic division at right angles to the plane of

the bed, but it is actually divided into columns as regular as those which are found in the trap rock, and bearing an average diameter of about two feet. On examining the transverse sections of these columns, it is found that many of them are marked by concentric lines of red and white, alternately preserving a circular form near the axis of the column, but gradually losing it so as to become angular towards the margin where the approximate columns begin to interfere with each other. Even in some part of this sand-stone bed, where no actual division into columns has taken place, the same sets of concentric groups of lines are to be found; each set deviating from the circular form as it begins to interfere with the surrounding ones, and indicating a future possible division of the whole into prismatic shapes. Here no action of the atmosphere can be supposed to have produced this effect, which, on the contrary, marks an original concretionary disposition in the rock, analogous to that which, in all probability, exists in the columnar traps, but here rendered visible by the differences of colour, as it is there concealed by the uniformity of tint.

Having thus proved that the spheroidal exfoliation of trap may proceed from a peculiar concretionary structure, as has been generally received among geologists, I was desirous of ascertaining whether cases might not also exist in which, as in the artificial forms of granite before described, it was due to the action of the atmosphere alone, and independent of any internal arrangement of the parts of the rock. It was evident that this could only be done by examining blocks which had received their forms by art, or by accidental fractures, and after no long search with this view, numerous opportunities were afforded in several places where such blocks of basalt and greenstone had been broken for the purposes of building agricultural dykes.

In many cases of trap, as of granite, no exfoliating tendency seems to exist, and therefore it may often be fruitless to search for illustrations of this subject. But in those to which I now allude, it invariably happened that the blocks used in the buildings, which, from their antiquity, or other causes, had sub-

sequently been demolished, showed the same tendency to exfoliation on all the exposed surfaces, into whatever forms they might have been broken, and whatever their size might be. Thus, in the smaller fragments, the process had already been carried so far as to have solid balls covered with a succession of crusts easily detached; while, in the larger, it was still going on, and promising ultimately to proceed to the same termination. It is plain that, in the case of irregular fragments so formed, no concretionary structure could be suspected; as it is not within the limits of possibility that they should have been broken from the larger masses in such a fortuitous manner as that the centre of the fragment should have coincided with a concretionary centre.

It must therefore be concluded, as enumerated at the commencement of this division of the subject, that in trap as well as in granite, two causes may act in producing the spheroidal desquamation, namely, an internal correspondent structure, aided probably by the action of the atmosphere, and the operation of that agent itself on the exposed surfaces of the same rocks, independent of any such original arrangement.

Having in the former part of this paper examined the nature of schistose or exfoliating granite, and attempted, although fruitlessly, to determine to which of the two cases under consideration this effect was to be attributed, it will render this subject more complete, to extend this inquiry to the case of schistose, or straight laminar trap rock. If, in this case, also, it appears as yet impossible to assign in all cases the true cause, the investigation will not be without use. No distinct account has yet been given in geological writings of this curious and important circumstance in the history of the trap rocks, nor have the various forms under which it appears been described; while it is also certain that some mistaken views have been entertained respecting it, as if it was limited to one species of trap in which it formed an essential character, and which has been very improperly distinguished by this casual and merely incidental circumstance, so as to be erected into a species under the name of porphyry slate. A more accurate description of these ap-

pearances will induce geologists to turn their attention to the actual investigation of that which they have hitherto taken for granted without examination; and possibly enable them to assign the true causes of those circumstances which the writer of this paper professes himself unable to explain.

I have here used the general term trap, merely to avoid circumlocution, as including, together with the greenstones and basalts most commonly designated by this name, all the overlying unstratified rocks of whatever nature. It thus comprises even the porphyries and syenites; but the names of the several species are also given wherever it has appeared necessary to speak more definitely respecting them.

The tendency to schistose, or flat laminar exfoliation on the surfaces, is more common in the rocks of this extensive family, than the spheroidal, since it occurs in every species that I have examined, whereas the latter is rarely found except in basalts and greenstones. An internal flat laminar structure, which seems independent of the agency of the atmosphere, seems also to exist in a greater number of species than the spheroidal concretionary form does, and I shall first describe the cases in which this modification occurs.

I have as yet observed examples of a flat laminar structure pervading a whole mass, only in veins and in prisms; and, in no instance, has a similar appearance occurred in amorphous masses where it was not referable to the other cases of superficial exfoliation, since it never penetrated deep into the mass. It is not uncommon in the veins of porphyry with a base of compact feldspar, which seem chiefly to exist in the primary strata; and, in those, as it occupies the whole breadth of the vein, to which it is moreover parallel, it appears independent of the action of the atmosphere. It occurs also in veins of basalt and of greenstone; and, in some of these cases, veins of very considerable breadth are found to put on this appearance throughout. In all these cases, the thickness of the laminæ varies from the eighth to the third of an inch, or more; and they are separated from each other more or less perfectly, but always without any signs of intermediate decomposition. Although no change

of texture is visible between approximate laminæ, this structure seems to bear an analogy to that larger laminar structure not unfrequent among these veins, in which the several laminæ present different characters; some being plain, others porphyritic, and a third set amygdaloidal. As no action of the atmosphere can be considered capable of producing the effect of a laminar division, thus deep seated and parallel to the walls of a vein, and on account of the analogy just noticed, there is reason to conclude that all these cases are true examples of a concretionary laminar structure.

The most remarkable instance of a flat luminar structure pervading a mass of prismatic form, occurs in Sky, in a greenstone of fine texture; and, in this case, the division is parallel to the axes of the prisms. It bears no resemblance therefore in form to the spheroidal exfoliation of columns formerly noticed; as it does not take place at all on the transverse surfaces, while it also occurs, in greater or less perfection, to such a depth within the stone, as to be apparently free from the influence of external causes. It must also be remarked, that, both in this instance and in that of the laminar veins, the integrity of the structure is in no way affected, but that the laminæ retain even the original colour of the massive parts of the rock.

It appears therefore reasonable to conclude, that, in this case also, as in the former, the laminar division parallel to the axes of the columns, depends primarily upon an original structure of that nature, which is developed, in all probability, by some action of air or water, or both, analogous to that which, in some of the cases formerly enumerated, seems capable of producing it upon the exposed surfaces.

I must now enumerate those cases of a laminar exfoliation, of much more frequent occurrence, the cause of which is far less easy to assign, and which, although in some instances they may be the consequences of a peculiar original structure, appear often to be produced, as in some of the cases of granite and others of spheroidal exfoliation in the trap rocks, by the mere action of the atmosphere on the flat exposed surfaces.

This phenomenon may be witnessed in so many parts of the

western islands of Scotland, that it is unnecessary to particularize the instances. I have observed it in clink-stone and in clay-stone, as well as in the various porphyries which have these ingredients for a base, and in the syenites.

In one or two instances it occurs in a clay-stone which has a columnar form; and here, contrary to the case described as found in Sky, the laminar exfoliation is at right angles to the axes of the prisms. It is proper to remark, however, that in the only examples which came under my notice, the formation of the laminæ was superficial; there being rarely more than the indication of three or four divisions, and not above one or two of these being so far separated as to admit of being removed. In no one case of this, or of the following occurrence of a laminar exfoliation of shapeless masses, did I observe that the deeper parts of the rock were affected, or that this structure could be inferred to pervade the whole.

In the above-mentioned cases also, where the exfoliation took place at right angles to the axes of the columnar claystones and porphyries, it is proper to remark that these were the only surfaces exposed. Yet it is not improbable, that future observers will find that the sides as well as the ends of such columns may present this appearance, to whatever cause it may be owing, whether to simple exposure or to an internal corresponding structure.

As the rocks now enumerated are much more commonly found in an irregular than in a columnar form, so the instances of schistose exfoliation are sufficiently frequent in these to admit of their examination in almost all situations where rocks of this nature occur.

The only one of these rocks which presents a large laminar disposition analogous to that of granite, is hypersthene rock. In this, the exfoliation in question sometimes occurs on the surface; but it is always limited to one scale, which, in its thickness and tenacity, and in the general appearance at the plane of separation, exactly resembles that which occurs both in the schistose granite of Arran, and in the columns from Leptis, already so often mentioned.

Few marks of such a large laminar disposition are to be observed in the syenites, clay-stones, and clink-stones, or the porphyries derived from these two latter rocks; although the Isle of Mull presents some remarkable exceptions. No relation can therefore be inferred between the direction of the exfoliation and that of a larger mass; and accordingly we can only conclude that, whatever surface is thus found exfoliating, the direction of this change relates to that of the exposed surface. Yet, as it will, I believe, always be observed, that none of these rocks exfoliate in two directions at any one place, it is probable that this tendency does actually bear some relation to the internal structure of a mass, no other indications of which are visible.

The number of successive laminæ which may be detached from any of the rocks last named, is various; but I know not that it bears any constant relation to the species. I have only observed, that when they do occur, they are more numerous in the softer clay-stones than in the syenites, or rocks with a base of clink-stone. If one, two, or three, can be detached in succession, that facility soon ceases; and, after some partial indicatures of future desquamation, the rock is found to be massive, and to give no marks of the future renewal of a similar process, although there can be little doubt that it will continue in succession as the preceding laminæ become detached. Such laminæ generally present the same average but variable thickness which has already been mentioned. The surfaces are also occasionally undulated or irregular, and, in a few instances, slightly curved. From preserving the tenacity of stone, it is generally supposed that they are unchanged, and present the natural characters of the rock to which they belong. But this is not the fact; and they will be found, in this important circumstance, to differ essentially from those laminæ already described which pervade the interior of veins, and which are conceived to arise from an internal concretionary structure. The deception has in these cases arisen from the great depth to which the rocks of this family sometimes weather without losing their tenacity, undergoing little apparent change but that of colour. Hence the solid parts of such rocks, when broken even

below the exfoliating surfaces, will be found to present the same appearance as the detached scales. But if a deeper section be made, it will, I believe, invariably be found that the natural state of the rock is different, and that the process of exfoliation has been attended by a partial decomposition or change.

I may here briefly remark, while on this subject, that the term *schistose porphyry*, already noticed as applied to the rocks of that character which are susceptible of this change, is injudicious. Such a term might, even with more propriety, be applied to the laminar porphyries with a base of compact feldspar that occur in veins; since in them it appears more truly a natural conformation, and independent of the action of extraneous or accidental causes. But in any case it is improper to erect a species from distinctions which are merely of an accidental nature, and which, if in this case they are not absolutely produced, are still materially influenced by accidental causes. Nor is there any reason why the term schistose should be limited to the porphyries alone that present this character; as there is the same reason for extending it to the clay-stones and syenites that exhibit the same appearances. I have here used the term schistose porphyry in making these animadversions. That of porphyry slate, commonly in use, is still more objectionable; but I need not prolong these remarks, having already noticed this circumstance in my Account of the Western Islands.

It only remains, in concluding this subject, to inquire whether the exfoliation of the laminæ, in these last examples, depends, as in the case of artificial blocks of trap and granite already described, upon the action of the atmosphere alone, or whether it is the consequence of this action coinciding with an internal and original concretionary arrangement. On this subject, it appears to myself that the arguments on each side are pretty nearly balanced. A similar structure of a concretionary nature is proved to exist in some traps, and therefore it may be conceived to exist in these also. On the other hand, it takes place only within the influence of the atmosphere, and is attended with a degree of decomposition which indicates the

influence of air and water on the rock. It seems more prudent to leave this question undecided for future examination; and I shall therefore terminate this paper, satisfied with having placed the facts in such a state as to be ready for any future illustration which geologists may add to the subject.

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Additional Remarks on the Desquamation of Rocks.

When the foregoing paper was written, I had not observed the desquamation which is there described, in any rocks but those of the families of the Granite and Trap. But an interesting case of this nature having since occurred in one of the primary stratified substances, it has appeared to me worthy of being added as a supplement to that record. It is true that it does not throw any additional light on the immediate cause, or on the mode in which the atmosphere operates in producing this remarkable effect. Yet it proves clearly, as clearly as the case of the columns of Leptis, that this effect is not dependent on any internal concretionary structure in the stone, and that it is not even limited to those rocks which are so often characterized by that peculiarity.

In Glen Almond, a well-known valley, through which the military branch of road from Stirling to Dalnacardoch passes, and which conveys the river Almond from its native hills to the great plain of Strathearn, there is an insulated stone long known by the name of Ossian's Tomb. Whatever interest this belief may excite in the breast of a true Highlander, a geologist is very little concerned with respect to that visionary claim of honour; but, from some indications of a circle of which it is the centre, it appears rather to have been adopted as the central stone of a Druidical structure. From its weight and bulk, it has unquestionably not been placed by art in its present position; but appears, on the contrary, to be a block which, like many others, has fallen from the impending precipices of the hills above.

It corresponds in its nature with these; being a micaceous

schist of a foliated and somewhat undulated structure, and being also characterized by a considerable quantity of quartz proportioned to the mica which enters into its composition. It is a very hard variety of this rock; the quartzose laminæ being compact and of considerable thickness.

From the universally weathered state of all the exposed surfaces, and from the succession of lichens which appear to have grown and decayed on it, a long period must have passed since it quitted the mountain above: if ever applied to the objects of Druidical worship, as is here supposed, we can at any rate assign pretty nearly the minimum of time during which it has lain where it now stands.

Although this stone bears, on all the surfaces, those slight marks of decomposition so well known in micaceous schist, which consists in the rusting of the iron of the mica, or in its assuming a brown hue, it is only on one side that the peculiar effect which forms the object of this paper has taken place. This is the north-eastern, or northern quarter; and, from the form of the valley, it is probably that on which the wind and rain beat with most violence. This circumstance furnishes a proof, if proof were wanting, that the effect, however inexplicable, is the result of atmospheric action.

From nearly the whole of this surface, scales of the rock can be detached, scarcely differing in tenacity and hardness from the original stone, and thus resembling those which have exfoliated from the granite columns described in the former paper. Their thickness varies in different parts, from the sixth of an inch to nearly the half; and they may be obtained, by careful application of a knife or thin chisel, in plates containing from six or eight to twelve or sixteen superficial inches. At the planes where they separate from the mass of the rock, no powdery matter, or grains of sand, or rusty clay, or any other marks of real decomposition are visible; both the surfaces being clean and smooth, as if cut by a sharp tool.

It must further be observed, that there is here a succession of similar operations visible. At some former period, the plates first detached have in some places fallen off, so as to

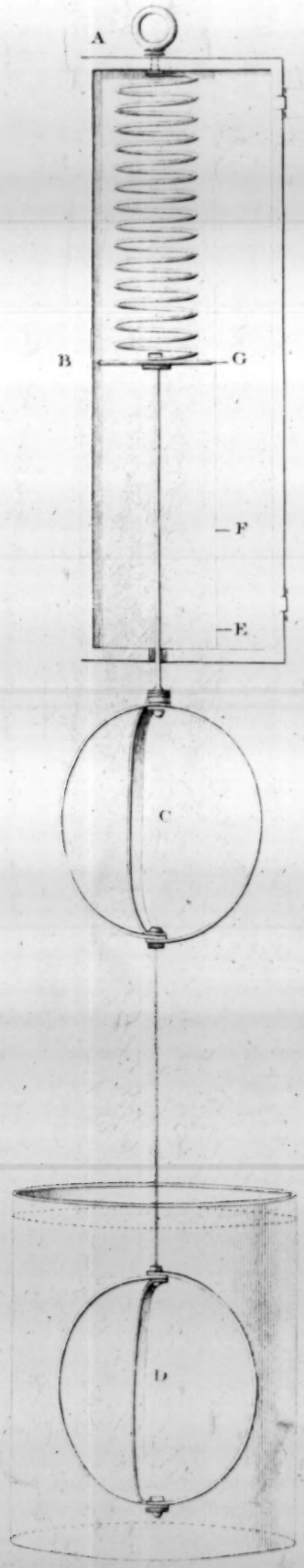
leave a new surface exposed; and on this the same desquamation has again taken place, so that, in some parts, two successive plates can be separated. The second plate is, however, in this case, not placed under the first, but forms a lower stage or step; its upper surface, if prolonged, being the under one of the first scale. In other cases, it is merely apparent that the process is about to be completed at some future period; the edge of the scale just admitting a knife, but it being as yet possible only to detach a small portion at the edge.

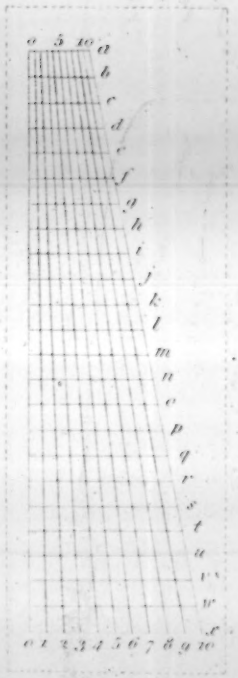
In one place it was also observed, that there was an incipient mark, or indication of a third scale; leaving no doubt that, in a sufficient length of time, the same process may be expected to take place through the whole block, should the same external circumstances continue to act.

This fact would have been in no respect remarkable, if the desquamation had taken place in a direction parallel to the laminar structure of the stone. Where micaceous schist is of such a nature that laminæ of quartz are separated by others of mica, the rusting of the iron in the latter, and its consequent decomposition, easily permit the quartzose laminæ to be detached. But, in this case, the desquamation is at right angles, or, at least, at considerable angles to the laminar structure. Thus it produces a scale, or slate, which consists of parallel bands of quartz and mica; both of them remaining unchanged, as in the solid rock, but easily separated in consequence of the fragility of the micaceous band.

It is evident, therefore, that in this instance the desquamation is not only not produced by the peculiar structure of the rock, but is utterly independent of it. Under the same circumstances, it might equally be expected to occur, either in a mass of pure quartz rock, or in a micaceous schist of a more simple and homogeneous nature; and, in this latter case, the schist might desquamate at angles to its fissile tendency. Such an occurrence might excite surprise; but there is no apparent reason why it might not happen in either of these rocks in a separate state, as it does here where they are intermixed in distinct laminæ.

The peculiar circumstances of predominant exposure to the





Mr. Creighton's Instrument for ascertaining Specific Gravities.

weather on the one side of this rock, where the desquamation takes place, contributes to prove, still more clearly than in many of the instances formerly enumerated, that the whole of this process is caused by the action of the atmosphere and the rains. However mysterious it may at present appear, it is the result of some chemical actions which cannot for ever be concealed. How far it may be connected with any other circumstances of the same nature more generally interesting, it is impossible to foresee. But, like all new facts in an obscure science, it is worthy of record. In multiplying the examples of difficulties and obscurities, they become gradually removed from the list of exceptions; while the varieties which are discovered in them on the comparison of many examples, sometimes point out the causes which have influenced the whole.

Art. II. *Account of an Instrument for determining the Specific Gravities of Solid Bodies.* By H. Creighton, Esq.

[In a Letter to the Editor.]

Sir,

Some time ago, I constructed an instrument for determining the specific gravities of solid bodies, of which the following is a description: I presume the same may be found acceptable to some of the readers of the Quarterly Journal.

The theory of this instrument depends upon the known property of springs, undergoing degrees of extension proportional to the weight or force applied to them, and the screw form usually adopted for weighing of bodies is best adapted to the purpose now in view.

In the accompanying sketch A. B. Plate IV. represents one of these springs suspended at A, and carrying an index at B. its lower extremity: C. D. two cages of curved wires, with joints to fold into a small compass, (or scales similar to those usually attached to the ends of a balance-beam may be used instead); the lower one D. is immersed in a vessel of water (which must be considerably deeper than represented) and both are sus-

pended by fine threads or wires from the end of the spring at B.

The substance whose specific gravity is to be ascertained being placed in C, it will stretch the spring and lower the index, suppose to E, where a mark is to be made; let the body then be transferred to D, and a second mark F. made opposite to where the index now stands. G being the point shewn by the index when the scales are both empty, G. E. ($= \alpha$) will represent the whole weight of the above substance in air; G. F. its weight in water, and E. F. ($= \beta$) the difference of these weights; consequently $\frac{\alpha}{\beta} = S$, the specific gravity required. To find the value of S, take the extent E. F. with a pair of compasses, and by means of them see how oft this is contained in G. E. ascertaining the value of any fractional part by means of a sector, or a scale of diverging lines as represented at H. On a scale of this kind a horizontal line, as k, will be found, equal to the divisor E. F. divided decimally, and with subdivisions, if of sufficient magnitude; here the amount of the above fraction will be .71, and E. F. being twice contained in G. E. 2.71 will be the value of S, or the specific gravity of the substance experimented upon.

Extreme accuracy is perhaps not to be attained by an instrument of this kind without making the spring of an inconveniently large size; but for all common and generally useful purposes it may be employed with advantage. If the spring is of such dimensions that half a pound will without injuring it stretch five or six inches, an error of one in five hundred can scarcely occur, if common care is taken in ascertaining the result.

In the usual way of taking specific gravities, the imperfections of balances sometimes employed, the chances of error in noting down the weights, and in the consequent calculations, however simple, frequently render repetitions necessary, and employ a considerable portion of time. With an apparatus as described above, time will be saved; and as the results are made obvious in a great measure to the eye, as well as to the

understanding, any errors of magnitude are less likely to be made.

I constructed one of these instruments with a spring capable of sustaining about one pound when it was extended six inches. Another required four to five ounces to stretch it through the like space, and might be loaded with six or seven without injury. A third, with spring made of small harpsichord wire, stretched about six inches with 50 grains, and was extremely sensible.

The stiffness of these and similar springs depends not only upon the thickness of the wire of which they are made, the diameter, and the number of the coils, but also in a great measure upon the degree of hardness or temper given to the steel or other material used in their manufacture. The following table (I.) shews the result of some experiments I made with a variety of springs of steel of the same quality, and equally tempered, or as nearly so as could be effected by tempering the whole at one process; the wire was 0.045 inches diameter.

Column 1 is the mean diameter of each spring in inches, and column 2 the number of coils of wire when the springs were in their natural state or not stretched; column 3, the space in inches through which the extremity of each spring was extended by a weight of 10,000 grains.

TABLE I.

1	2	3
0.41	34.60	0.847
0.45	31.55	0.765
0.60	23.70	2.250
0.70	20.03	2.005
0.80	17.75	2.650
1.22	11.65	7.410
1.42	10.00	9.880

In Table II, (arranged as the last) the springs were of highly tempered steel; in Tables III. and IV. the springs were of hard iron wire, and sustained the weights given without injury.

TABLE II.

With wire 0.06 diam. the weight 30,000 grains.		
0.45	17.00	1.20
0.72	10.90	3.20

TABLE III.

With *iron* wire, .1175 diameter, the weigh 28lbs. Avoidupois.		
0.61	34.00	0.73
1.18	17.60	2.60
2.06	10.01	10.00

TABLE IV.

With *iron* wire, .221 diameter, weight 56lbs. avoirdupois.		
1.60	60.00	5.05

In preparing these springs, care was taken to keep the different coils of each a little asunder; they would not stretch uniformly if allowed to be in contact any where.

The space through which a given weight stretches any of these springs is in proportion to the number of coils contained in it, the diameter being the same; but where the diameters are various, there does not appear to be any regular proportion, though in some cases it was observed to be nearly as the squares of these diameters; the tables, however, may assist in giving a general idea of the size of springs required for any given weight, and in an instrument to be used for weighing or taking specific gravities, as above described, two or three springs might be contained, suited to different weights and divided scales of ounces, grains, &c., added to make it useful as a common spring balance; a very small box or case would contain the whole, and the scale of diverging lines may be put inside the lid or cover.

I have the honour to be, Sir,

Your very obedient Servant,

HENRY CREIGHTON.

W. T. Brande, Esq. R. I.

Art. III. *A Review of some of the General Principles of Physiology, with the practical Results to which they have led*. By* A. P. W. Philip, M. D., F. R. S. Edinb.

[Continued from page 113.]

We are now to direct our attention to the phenomena of the nervous system. Under this term is generally included the sensorial as well as nervous system, properly so called. From a careful review of the functions of these systems, however, it will appear, I think, that they do not differ less from each other than from the muscular system.

M. le Gallois, as far as I know, is the only author who has endeavoured by experiments to draw a line of distinction between them. It is unnecessary, however, to examine the opinion he has advanced, as many of the facts which I shall have occasion to state will be found incompatible with it. After reviewing the phenomena of the nervous system, properly so called, we shall be better prepared to enter on this question.

The functions of the nervous are much more complicated than those of the muscular system. The first we shall consider is one on which I have already been necessarily led to make some observations. We have seen that the influence of the nervous system is the only stimulus of the muscles of voluntary motion, and that it is also capable of exciting those of involuntary motion, although in their usual functions the latter are excited by other means. Here the question arises, if the nervous system be not concerned in the usual functions of these muscles, why are they universally subjected to its influence? This question we are not prepared to consider till we have taken a view of some of the other functions of the nervous system;

* Errata in the first part of this paper:—Page 97, l. 22, *for* matter *r.* them. Some infer from the evident dependence of the phenomena of the vital principle on the due mechanism of the living body, that it is the immediate result of that mechanism; but this dependence would not be less, were the vital principle something superadded to bodies, as electricity and magnetism are by many supposed to be. There is nothing in the phenomena of the vital principle which can authorize either inference. Page 110, l. 4, *for* produce *r.* produces.

but the influence of that system not only does not excite the muscles of involuntary motion in their usual functions, but, as I have already had occasion to observe, is communicated to them in a way different from that in which it is communicated to the muscles of voluntary motion. We shall here inquire in what this difference consists.

The following positions have been ascertained by repeated experiments*. Chemical agents, applied to the brain and spinal marrow, more powerfully influenced the heart than mechanical agents, while the latter influence the muscles of voluntary motion more than chemical agents. Both, applied to the brain and spinal marrow, excite the heart after they cease to produce any effect on the muscles of voluntary motion. Applied to any part of the brain and spinal marrow, they affect the action of the heart, while the muscles of voluntary motion are only affected when they are applied to the parts from which the nerves of those muscles originate. Applied to the brain and spinal marrow, they never excite irregular action in the heart, while nothing can be more irregular than the action they excite in the muscles of voluntary motion. Their effect on these muscles is felt chiefly on their first application, but continues on the heart, within certain limits, as long as they are applied. These differences in the effects of agents applied to the brain and spinal marrow must, it is evident, be explained, before we can understand the relation which subsists between the nervous and muscular systems.

It appeared to me probable, from the result of several experiments, that the cause of chemical agents, applied to the brain and spinal marrow, producing a greater effect on the heart than those which act mechanically is, that the former, from their nature, act on a larger surface. If this opinion be correct, the mechanical agent, it is evident, may be rendered the most powerful, by confining the chemical to a smaller space than it occupies, which was found from frequently-repeated experiments to be the case†.

Most of the experiments on this part of the subject, it may be observed, as well as many to which I have already referred,

* *Exper. Inq.*, Part II., Chap. 4. † *Ib.* Exper. 41.

were made, not on the living, but newly dead animal, which was always employed if the nature of the experiment admitted of it*.

It appeared, from repeated experiments, that neither chemical nor mechanical agents, applied to the brain and spinal marrow, affect the action of the heart, unless they make their impression on a large portion of these organs. Every part of them may be stimulated individually, without the action of the heart being influenced; and the agent being the same, its influence on this organ is always proportioned to the extent of surface to which it is applied†. It does not appear that it is of much importance on what part of the brain the agent makes its impression, Even stimulating the surface alone, either mechanically or chemically, immediately increases the action of the heart.

Another circumstance, which appears to be of great consequence in explaining the difference of the effects of agents applied to the brain and spinal marrow on the two sets of muscles, is, that the heart obeys a much less powerful stimulus than the muscles of voluntary motion do. The most powerful chemical agents alone affected them, while all that were tried readily influenced the action of the heart ‡. Mechanical agents which, by bruising and dividing the parts, occasion the greatest possible irritation, are best fitted to excite the muscles of voluntary motion. Chemical agents, indeed, from their effects on the heart, we should, at first view, consider the most powerful. But their greater effect on this organ is readily explained by what has just been said. When the effect of the mechanical agent was rendered extreme and general, its influence on the heart was found much greater than that of any chemical agent which was tried. We have seen that suddenly crushing any considerable portion of the brain or spinal marrow instantly destroys the function of that organ.

The conclusions, then, at which we arrive are, that the heart

* Why the newly dead animal is as good a subject for many physiological experiments as the living one, will appear from what I shall afterwards have occasion to lay before the reader.

† *Exper. Inq.* Exper. 41, 42, 43, ‡ *Ib.* Exper. 35, 36, 37.

is influenced by all agents applied to any considerable part of the brain or spinal marrow, while the muscles of voluntary motion are only influenced by the more powerful agents applied to certain small parts of them.

These facts being ascertained, the other differences observed in the effects of agents applied to the brain and spiral marrow on the heart and muscles of voluntary motion are easily explained.

Irregular action of a muscle arises from stimuli acting partially or at intervals on its nerves, or on the part of the brain or spinal marrow from which its nerves arise; but very partial action of an agent on these organs, we have just seen, is incapable of exciting the heart; and while the agent is applied to any part of them, as all their parts seem equally to influence the heart, it cannot act upon it interruptedly, as an instrument does on the muscles of voluntary motion, when it is moved from place to place in the brain*.

The heart feels the effect of the agent within certain limits, as long as it is applied to the brain and spiral marrow, while the muscles of voluntary motion chiefly feel its first impression; because they feel only the effects of powerful agents, applied to certain small parts of these organs, which, being strongly impressed, soon lose their excitability; while the heart feels the sum of all, even slight impressions, made on every part of them†.

It also appears, from what has been said, why those who have endeavoured to influence the heart by stimulating the small parts of the brain from which its nerves seem chiefly to originate,

* It is true, that although the heart is only influenced by agents applied to a large portion of the brain, we may conceive them so applied as to produce irregular action in it, and we find that certain irritations of the nervous system have this effect. Suddenly crushing part of the brain or spinal marrow renders the action of the heart irregular. But it is evident that the heart, not being subject to agents whose action is confined to a small portion of these organs, and being equally affected through all parts of them, must render it much less subject to irregular action, and readily accounts for this not having been observed in the experiments just referred to.

† *Exper. Inq.*, Exper. 34.

have failed; and why the heart may be influenced through this organ and the spinal marrow, after their power is too far reduced to excite the muscles of voluntary motion*. As these only obey agents applied to one part, if the change there be not sufficiently great to produce the effect, it can be assisted by no other. Thus I have found by experiment, that a blow which affects the brain generally, without materially injuring it, produces comparatively little effect on the muscles of voluntary motion, but it produces a great effect on the heart, because it feels the sum of all the impressions. The nervous system, therefore, may be so far exhausted as not to admit of the vivid impressions necessary to excite the muscles of voluntary motion, and yet capable of those which influence the heart.

The heart, however, is not the only organ which receives nervous influence from every part of the brain and spinal marrow. The power of the blood vessels, we have seen, may be destroyed by the sudden destruction of either of these organs, and it also appears, from direct experiments†, that they may be influenced, even in the extremities, by agents applied even to the upper surface of the brain. The alimentary canal may also be influenced through both the brain and spinal marrow. From the extreme irregularity of the motions of this canal, we cannot ascertain whether it is subject to the influence of the different parts of the brain and spinal marrow, in the way in which this was done respecting the heart and blood vessels. I therefore endeavoured to ascertain this point by experiments of a different kind; from which it appears, that on withdrawing a great part of the influence of either the brain or spinal marrow, the stomach is affected in a way which I shall soon have occasion to consider more particularly‡.

We may easily conceive why the muscles of voluntary motion are excited when those parts of the brain or spinal marrow from which they receive their nerves are stimulated; but it seems at first view more difficult to account for the heart and other

* *Exper. Inq.*, Exper. 38.
† *Exper. Inq.*, Exper. 26, 27.
‡ *Ib.*, Chap. VII. sect. II.

muscles of involuntary motion being subject to the influence of every part of these organs. We cannot suppose that they receive nerves from every part of them. We know, indeed, that no organ does so. The following seems to be the state of the question. We see some parts influenced by every part of the brain and spinal marrow; others only by small parts of them. In the latter instances, we see directly proceeding from those small parts the nerves of the part influenced. In the former instance, namely, where the part is influenced by all parts of the brain and spinal marrow, we do not see nerves going directly from all parts of these organs to the part influenced, but we see this part receiving nerves from a chain of ganglions to which nerves from all parts of them are sent. It is, therefore, evident, from direct experiments, that the nerves issuing from ganglions convey to the parts, to which they send nerves, the influence of all the nerves which are received by these bodies.

Such then is the relation which the most important organs of involuntary motion bear to the brain and spinal marrow. Their powers are not directly dependent on either, yet they are subjected to the influence of every part of both, communicated through the medium of the ganglions; and when we see the other organs of involuntary motion equally independent of the brain and spinal marrow, and supplied with nerves from ganglions, in the same way with the former, it is impossible not to infer, that they bear the same relation to the nervous system. Thus, it would appear, that the ganglions may be regarded as a secondary centre of nervous influence, receiving supplies from all parts of the brain and spiral marrow, and sending to certain organs the influence of all those parts.

If the nervous influence of the thoracic and abdominal viscera be thus supplied from a common source, why, in affections of the spinal marrow, it may be asked, is the breathing most influenced when the disease is in the dorsal portion of this organ, and the action of the bladder and rectum, when its chief seat is in the lumber portion? This necessarily arises from the intercostal muscles deriving their nerves from the dorsal, and the abdominal muscles from the lumber portion of the spinal mar-

row. The latter muscles generally excite, or, at least, increase, the action of the bladder and rectum, by pressing them against their contents, and also by this pressure contribute mechanically to expel their contents. Thus, in the above cases, in addition to the failure of nervous influence in the viscera, there is a failure of excitement in the muscles of voluntary motion which conspire with these viscera in certain parts of their functions.

We can trace the communication of nerves issuing from the great chain of ganglions, placed it would seem, to facilitate these communications in the centre of the animal system, with all the nerves of the body. Bichat, although his opinions respecting the use of the ganglions are inconsistent with the results of the experiments just referred to, as well as of others to which I shall have occasion to refer, was induced, from their situation and the distribution of their nerves, to regard them as the centres of nervous systems.

On comparing all the facts on the subject, we have reason to believe, that the system of ganglionic nerves is quite as extensive as that of the nerves proceeding directly from the brain and spinal marrow. We every where find blood-vessels which, being influenced equally through the brain and spinal marrow, must receive the nervous influence through the ganglions; and, indeed we can trace the ganglionic nerves attached to and supplying the larger vessels. The following case, related by Dr. Parry, in his treatise on the arterial pulse, might alone be regarded as proving the existence of two sets of nerves in the extremities, the one supplying the organs of voluntary, the other those of involuntary, motion, and strikingly illustrates what has been said on this subject. He observes, "I have seen a total loss of pulse in one arm, with coldness, but complete power of motion in that part while the other arm was warm, and possessed a perfectly good pulse, but had lost all power of voluntary motion."

Such then is the manner in which the influence of the nervous system is supplied to the muscles of voluntary and involuntary motion. To the former, from certain small portions of the brain and spinal marrow, and through nerves going directly from these

small portions to the muscles; to the latter, from every part of the brain and spinal marrow through a chain of ganglions which, on the one hand, communicate with every part of these organs, and, on the other, with all the muscles of involuntary motion; in the former instance, the influence of the nervous system being the only natural stimulus, in the latter, other stimuli exciting the muscle to its usual function, and the influence of the nervous system, being only occasionally bestowed on it for purposes which we shall soon have occasion to consider.

When the nerves of a muscle of voluntary motion are divided, the supply of the stimulus on which its function depends being cut off, it is rendered paralytic, not because its power is impaired, for it is as sensible to the effects of stimuli as while its nerves were entire, but because the channel of the only stimulus by which the will operates on it is obstructed, and here the effect of the division of these nerves ends. The consequence is very different, when the nerves of the muscles of involuntary motion, the ganglionic, are divided.

If the principal ganglionic nerves, the eighth pair through which the influence of the brain is chiefly supplied to those muscles be divided, the function of the muscles appears to be wholly unaffected by it. The heart and vessels support the circulation as well as before the division of the nerves. For this result we are prepared by what has been said of these muscles. An evident disorder, however, in the secreting power of some of the vital organs immediately ensues. The stomach no longer secretes a fluid capable of producing the necessary change on the food*, while the fluids of the lungs deviate from the healthy state, and accumulate in the bronchiæ and air cells. The structure of the lungs itself, in the space of a few hours, becomes evidently diseased, and the animal dies of dyspnœa; failure in the office of the lungs, necessarily proving more suddenly fatal than failure in that of the stomach. It has been questioned, whether the effects on secreting surfaces of dividing the eighth pair of nerves should be ascribed to the interruption of the

* *Exp. Inq.* Chap. 5.

influence of the brain, or to the injury done to those surfaces by the act of dividing their nerves*. That it produces its effects in the former of these ways, appears from the following facts. When secreting surfaces are deprived of their nervous influence by any other means, the effect is the same; this effect is not at all proportioned to the degree of injury done to the nerves, but to the degree in which the nervous influence is withdrawn; and, as soon as the nervous influence is restored, the surface is again capable of its function†. These facts seem sufficient to have answered the question, although it had not been determined by some late experiments, in which Mr. Brodie and Mr. Cutler were so good as to assist me‡, that it is necessary, after the division of the nerves, to displace one of the divided ends, in order wholly to arrest the function of the secreting surface, the influence of the brain still passing in such a quantity, if this be not done, as to bestow on that surface a considerable degree of the secreting power; and that even when the divided ends, if not otherwise displaced, are separated to a distance of a quarter of an inch.

Thus, we find, that the effect of dividing the ganglionic nerves is of a nature wholly different from that of dividing the cerebral, or spinal nerves; while the division of the latter only deprives the animal of the power of exciting the muscles of voluntary motion, that of the former deranges the functions on which its life depends. Even the structure of the lungs, we have just seen, is evidently disordered in a few hours by the division of the eighth pair of nerves in the neck.

As the function of the stomach is destroyed when the influence of the brain through the eighth pair of nerves is cut off, we should at first view infer, that it is from the brain alone that the stomach derives its nervous influence. But although the process of digestion be suspended by the division of these nerves, it does not follow that the stomach may not derive nervous influence from some other source, because the loss of any considerable part of this influence may destroy its function. Besides,

* Journal of the Royal Institution, No. 17.

† *Ib.* No. 18, page 257.

‡ *Ib.* No. 23, page 17, *et seq.*

its remaining sensibility, indicated by the efforts to vomit, proves that the influence of the nervous system is not wholly withdrawn from it by dividing these nerves.

If, then, this influence be not supplied to the stomach by the eighth pair of nerves alone, but also, as we have reason to believe from the evidence of anatomy, by nerves arising from different parts of the spinal marrow, it is evident that cutting off the influence of any considerable part of this organ, while we leave the eighth pair of nerves entire, must affect the power of the stomach, though probably not so much, because the brain, we have reason to believe, constitutes the most important part of the nervous system. The same observation applies to the lungs. On appealing to the test of experiment, such was found to be the result; the functions both of the stomach and lungs were impaired, by destroying any considerable portion of the spinal marrow, the lesion of function being proportioned to the extent and importance of the part destroyed*.

Another point relating to this part of the subject remains to be ascertained. Do the effects observed in the stomach and lungs, when part of the spinal marrow is destroyed, arise directly from the destruction of that part, that is, from the ceasing of its office, or from the influence of the brain on the spinal marrow being thus limited? It is evident, that if the former opinion be correct, the division of the spinal marrow in the middle will not produce the same effects as the destruction of the lower half. If the latter, the effects must be the same. The division of the spinal marrow in the middle produced very little deviation from the healthy state, either in the stomach or lungs, compared with that produced by the destruction of the lower half of that organ†.

Thus, it appears, that the function of the spinal marrow also is necessary to the secreting power, and that, as far as it is necessary to this power, it is independent of any influence derived from the brain. As a partial destruction of the spinal marrow impairs the secreting power, a partial abstraction of the influence

* *Inquiry*, &c. Exp. 54, 55, 56.

† Exp. 59.

of the brain has the same effect. It was found that the division of one of the eighth pair of nerves deranges the function of both the stomach and lungs, nearly in the same degree with the destruction of a certain portion of the spinal marrow*. The function of every part of the brain and spinal marrow therefore is necessary to the due performance of secretion.

Here a question of great importance in the animal economy arises. As it appears, from the experiments just referred to, that the nervous power is equally essential with the circulation of the blood, for maintaining the functions of secretion and assimilation, what are the parts they severally perform in these functions? It is evident, that the extreme parts of the sanguiferous and nervous systems are connected in a way very different from that in which these systems are connected in other parts. The heart and vessels of circulation, we have seen, can perform their function after the influence of the nervous system is withdrawn. The function of the secreting vessels immediately ceases on the interruption of this influence. We must suppose, therefore, either that the influence of the nervous system bestows on the extreme vessels the power of separating and re-combining the elementary parts of the blood, or that the vessels only convey the fluids to be operated upon by this influence.

Experiments, to which I have already referred, prove that the most minute vessels which can be seen by a powerful microscope in the web of a frog's foot, are independent of the nervous system. The motion of the blood is as rapid, and the circulation in the foot presents precisely the same appearance after as before the slow destruction of the brain and spinal marrow. If the power of the vessels of secretion had been lost by the interruption of the influence of the nervous system, would not this have necessarily occasioned some change in the distribution and motion of the blood in the web? The conclusion from these experiments is strengthened by others. In those in which the secreting power was destroyed either by the division of the eighth pair of nerves or the destruction of part of the spinal marrow, there did not necessarily appear to be a de-

* *Inquiry*, Chap. vii. Sect. 2.

fective supply of fluids. In the stomach they were often as copious, sometimes more copious, than usual; and the latter was almost always the case in the lungs. The fault seemed to be, that a due change on them had not been effected. We know that the vessels of circulation possess no powers but the elastic and the muscular, or what in many of its properties resembles the latter. Can we suppose, that the vessels of secretion, which are only a continuation of those of circulation, all at once assume a different nature; or is it at all consistent with our knowledge of the phenomena of chemistry to suppose, that by any influence the powers just mentioned, or indeed any that can be supposed to belong to vessels, could be enabled to separate and re-combine the elementary parts of the blood? The first of the above positions being set aside, it seems a necessary inference from the experiments referred to, that in the function of secretion, the vessels only convey the fluids to be operated upon by the influence of the nervous system.

It is not to be overlooked, however, that the vessels convey the fluids in a peculiar way. By the lessening capacities of the capillaries, the blood is divided, as by a fine strainer, some of its parts being too gross to enter the smaller vessels. How far the blood may thus be subdivided we cannot tell. As this structure of the vessels is uniform, we have reason to believe, that its effect on the blood is necessary to prepare it for the due action of the nervous influence.

We are now prepared to consider the question, for what purpose is the influence of the whole nervous system bestowed on the muscles of involuntary motion? Admitting, it may be said, that the due performance of secretion requires the united power of all parts of the brain and spinal marrow, and that we may therefore explain why their united influence is bestowed on secreting surfaces; the question still remains, why should their united influence be bestowed also on the muscles of involuntary motion?

It is evident, that affections of the nervous system could produce no occasional increase of the secretions, were not the sanguiferous system, and particularly the vessels of secretion,

capable of being stimulated by the same influence which operates in the formation of the secreted fluids. The increase of secreting power in any part would be in vain, were there not at the same time a corresponding increase in the supply of the fluids on which it operates. A similar observation applies to the excretory muscles, as far as they are muscles of involuntary motion. The same increase of nervous influence which occasions an increased flow of secreted fluids, excites these muscles to carry off the increased quantity. Nature does not seem to trust this to the increase of stimulus, occasioned by the increased flow of the secreted fluid, which we have reason to believe from the *modus operandi* of certain causes of inflammation, would often occasion morbid distention. Now the vascular system, and the muscles of excretion, if in them we include the alimentary canal, comprehend all the muscles which are supplied with ganglionic nerves, unless we regard the iris as a muscle. The state of this organ is quite anomalous in the animal economy, being one of involuntary motion, excited only through the medium of the nervous system.

In the preceding view of the subject, we find the relation of the vessels of secretion to the nervous system the same as that of the heart and vessels of circulation. Their function is independent of, but capable of being influenced by, this system.

Thus, we perceive, the necessity of every part of the function which the ganglions appear to perform. A combination of the whole nervous influence is necessary to the due formation of the secreted fluids; and that there may be, under all circumstances, both a due supply of the fluids to be acted upon, and a due removal of those prepared, whether for the functions of life, or for the purpose of being thrown out of the system, it is necessary, as appears from what has just been said, that the powers which convey all these fluids should be subjected to the influence by which secretion is performed. This function, it is evident, requires a more regular supply of fluids than could have been obtained, had the usual action of the vessels depended on the nervous system, which is subject to continual variation; but had not this system been capable of influencing the vessels, not

only no change in it could have influenced the flow of secreted fluids, but every occasional increase of the influence of the nervous system, supplied to secreting surfaces, finding no increase of fluids to act upon, would necessarily have excited disease. Thus, it is requisite that the power of the sanguiferous should be independent of the nervous system, yet capable of being influenced by it; as from direct experiment, we have just seen, it is found to be.

The secreting processes are constantly attended with a temperature considerably raised above that of the surrounding medium. Does this also depend on a function of the nervous system?

It appears, from experiments above referred to, that the destruction of any considerable portion of the spinal marrow, deranges the function of secreting surfaces. Together with this effect, it was always found to lessen the temperature of the animal, more or less, according to the extent and importance of the part destroyed*. Some years previously, Mr. Brodie, in the Croonian Lecture for 1810, gave an account of experiments which led to the inference, that the maintenance of animal temperature is under the influence of the nervous system, and in the Philosophical Transactions of 1812, he relates additional experiments, tending to strengthen this inference. The experiments related in the inquiry just referred to, seem in a striking manner to confirm the opinion of Mr. Brodie. He found that poisons impairing the vigour of the nervous system, impair the temperature. It appears from my experiments, that lessening the extent of this system, by destroying part of the spinal marrow, has the same effect.

Thus, it follows, that the temperature of the animal body depends on the state of the nervous system; but many observations point out that it depends also on that of the powers of circulation. When the power of the heart and vessels is greatly impaired, so that the motion of the blood languishes, the temperature falls. If by exercise, or the use of stimulants, we increase the action of the heart and vessels, the temperature in

* *Exper. Inquiry*, p. 161, *et seq.*, second edition.

the same proportion rises. When there is a natural defect in the organs of circulation, and particularly when this defect is such as prevents the blood passing through the lungs, with the freedom necessary to its healthy state, the temperature is found below the natural standard. The reader may consult a paper on this subject, by Mr. Earle, in the seventh volume of the Transactions of the Medico-Chirurgical Society, in which there are many excellent observations. As we proceed, we shall find proofs founded on direct experiments, that the temperature depends on the state of the circulation, and particularly on the passage of the blood through the lungs, which to detail here, would too much anticipate some of the other parts of the subject.

Whether caloric be a substance, or as some of the first chemists of our time are inclined to believe, only a certain motion of the particles of bodies, it is of course foreign to this paper to inquire; but it appears from the foregoing observations, and will, I think, appear still more strikingly from those I shall have occasion to add, that the maintenance of animal temperature must be ranked among the results of the action of the nervous system on the blood. It is on this account that I have elsewhere said, *that if caloric be regarded as a substance,* its evolution in the animal body must be ranked with the secreting processes; the definition of secretion, I conceive, being the evolution of a *tertium quid,* in consequence of that action.

When to the functions which have now been detailed, we add, that the nerves are the means of conveying impressions to and from the sensorium, we have, I believe, enumerated the whole of the functions of the nervous system properly so called.

Although it has been very generally admitted, that the nerves of the organs of sense perform no other function but that of conveying to the more central parts of the nervous system the impressions they receive, it has been supposed that the nerves of other parts, and particularly those of the viscera, are capable of so impressing each other, that these parts sympathize independently of the more central parts of the system. This position, which, were it correct, would seem in opposition to

many established laws of the nervous system, I have considered at some length in the ninety-sixth and following pages of the second edition of my Treatise on Indigestion. It appears, as far as I can judge, from the facts there adduced, that it is altogether unfounded, the nerves seeming in the latter as in the former case, only to convey impressions made on their extremities to the more central parts of the system.

(To be continued.)

Art. IV. *On Soda-Alum*, in *a Letter from* Dr. Ure *to* Dr. Wollaston, V. P. R. S.

Glasgow, March 1, 1822.

Dear Sir,

I now fulfil the promise which I made at my visit to London, in October last, of sending you specimens of soda-alum, a salt, concerning whose existence, or possible formation, our ingenious friend, M. Clement-Desormes, expressed, as you may remember, the strongest doubts. The larger crystals were made by Mr. William Wilson, an extensive alum-manufacturer in this neighbourhood; the smaller and more regular ones were formed by myself in the following way:

In a cold lixivium of effloresced alum schist, of specific gravity, 1.35, previously freed by crystallization from the greater part of its sulphate of iron, I dissolved with trituration in a glass mortar nearly as much muriate of soda as the liquid would take up. The solution was then left to spontaneous evaporation, in a temperature of about 60° F. At the end of two or three weeks, abundant groups of regular octohedrons were found diffused throughout the yellowish pasty magma. These crystals were washed with alcohol, and dried with blotting paper. To free them entirely from an adhering portion of copperas, they must be re-dissolved and re-crystallized.

The form and taste of this salt are exactly the same as those of common alum; but it is less hard, being easily crushed between the fingers, to which it imparts an appearance of moisture. Its specific gravity was determined by weighing in

oil of turpentine. It is 1.6 reduced to water, as unity. 110 parts of it are soluble in 100 of water at 60°; and form a solution whose specific gravity is 1.296. In these respects, potash-alum is very different; for I find that of this salt, from eight to nine parts only are soluble in 100 of water at 60°, yielding a saturated solution, the density of which is no more than 1.0465.

One hundred parts of soda-alum crystals lose by a heat verging on redness 49, which seem to be merely water; for the residuary 51 parts re-dissolve in that liquid, without causing any sensible cloudiness. By test solution of muriate of barytes, I ascertained that 100 parts of soda-alum contain 34 parts of sulphuric acid. The resulting muriates being evaporated to dryness and ignited, weighed 23.3 of which 12.3 were found by the action of water to be muriate of soda, and 11 alumina. But 12.3 of muriate of soda are equivalent to 6.56 of that alkali.

To another 100 parts of this salt, I added water of ammonia, in slight excess, filtered, and exposed the well-washed alumina to ignition, after which it weighed 10.5 parts. The filtered liquid was evaporated to dryness and ignited. It weighed 14.4 parts, and was dry sulphate of soda; for, on re-dissolving it in water, and slowly evaporating the solution, crystals in grooved six-sided prisms were obtained, amounting to somewhat more than double the weight of the ignited salt; 14.4 parts of dry sulphate of soda are equivalent to 6.4 soda. From the mean of the above results, this soda-alum seems to consist of

Sulphuric acid,	34.00
Alumina,	10.75
Soda,	6.48
Water of crystallization,	49.00
	100.23

Had leisure permitted me to repeat the analysis with greater care, it is probable that the experimental quantities might have come out more nearly as follows:

Sulphuric acid,	33.96	—	4 primes	=	20.000
Alumina,	10.82		3	=	6.375
Soda,	6.79		1	=	4.000
Water,	48.43		25	=	28.125
	100.00				58.500

For certain purposes in calico-printing, this salt seems to be well adapted, by its remarkable solubility in water. That it contains no ammonia, I ascertained, by distilling a portion of its solution, off lime.

I am, Dear Sir,

Your most faithful servant,

ANDREW URE.

ART. V. *A Translation of* REY'S *Essays on the Calcination of Metals,* &c.

[Communicated by JOHN GEORGE CHILDREN, Esq., F.R.S., &c.]

Concluded from page 141.

ESSAY XXII.

It is not the volatile salt of the charcoal that increases the weight.

As soon as I had made the rough sketch of this discourse, I sent it to the person spoken of in the last Essay, who, a few days after, put into my hands a writing, disavowing the opinion which I have there combated; and, after having opposed to my creed respecting the thickening and increasing of weight of the heated air, the reasons which I have related in the 12th Essay, and refuted in that and the two following, he proposes his own opinion briefly as follows: The increase of weight is necessarily derived, either from the vessel, the air, or the charcoal. Not from the vessel, since it loses nothing of its weight; not from the air, since heat can only subtilize and render it lighter, as he pre-supposes proved; it remains therefore, says he, that it must be the charcoal to which it is owing. And, to show how that happens, he says that charcoal contains two parts or natures, one vegetable, the other metallic; and each of these two parts, one fixed, the other volatile. The

fixed part remains at the bottom of the furnace in the state of cinder, containing the fixed salt, which is separated by washing, and the volatile part rises all round the vessel, containing in a superfluous humidity (derived from the vegetable) a volatile salt, of a metallic nature; which, raised on high on the wings of the humidity, meeting the air directly over the vessel, which is more rarefied and lighter than the vapour of the charcoal, sinks through it into the vessel and attaches itself by a close sympathy to the fixed salt of the calx of tin, which having received a certain quantity of it, and being as it were saturated, rejects the surplus; like as salt of tartar, after a certain number of cohobations, cannot be further impregnated by the volatile salt, contained in the aqua vitæ. Having run over his writing, I refuted this opinion in his presence by the following arguments. Since we must give every one credit in his own art, if we have nothing to the contrary, it is reasonable, in speaking of volatile salts, to borrow the language of the alchymists, who alone can discourse of it in a proper manner, having first discovered it and revealed it to us, when we were not aware that there was a volatile salt in nature. They found in vegetables, as in almost every thing, two sorts of salts, one fixed, the other volatile; the former containing a fixed spirit in itself, and being contained in the solid part of its subject; the latter containing a volatile spirit, and being contained in the juices. The fixed is obtained, say they, by calcination, remaining after it in the ashes. The other cannot sustain the fire, being no less volatile in effect than in name, and thus rises with the slightest heat with the juice that contains it, or is lost even by simply drying the vegetable. Now this being so, it is out of doubt, that in the whole of the charcoal there can be no volatile salt, for even the wood of which it is made cannot contain any, since it is previously dried; and even if it did contain any, who but sees it must of necessity lose it, when in the charring it becomes a burning coal? Truly, were I to grant this favour, and allow that charcoal contains volatile salt, they who know how rare it is in every thing, will never believe that from the small quantity of charcoal which the Sieur Brun used

in his calcination, so great a quantity should have proceeded. For we must not only suppose seven ounces, but also that which has replaced the diminution of weight derived from the loss of vapours of the tin, and from its increased volume, and moreover, all that the fumes of the charcoal have carried off elsewhere, not only through the whole laboratory, but out of it, whither they have escaped in abundance by the openings. In which fumes, if there were salt in proportion to that which has sunk into the vessel, from those fumes that were above it, and it were all collected together, truly the collection would be monstrous!

And besides, when the calx of tin had taken its fill of salt, by this imaginary sympathy, what prevents the continuation of the fire from accumulating more above it, and filling the vessel, since it descends into it by its own weight? Experience refutes all this—besides that I have demonstrated, that the air above the vessel is so thick, that it could not descend into it. Moreover, if a furnace be constructed in a wall separating two chambers, so that the vessel may be on one side, and the registers and doors for putting in the charcoal and admitting air on the other, I maintain that the increase of weight will ensue, though no vapours can enter the chamber containing the vessel. This I have confirmed by proof, which I made in the forges of my elder brother, John Rey, Sieur de la Perotasse; when I found an equal increase in tin that I calcined, on what they call a *gueuse*, or ingot of iron, weighing from sixteen to twenty quintals, at the instant that flowing from the furnace it is thrown into its mould. Now it cannot be said, that the vapours of the charcoal contribute any thing in this case. Wherefore this volatile salt is inadmissible in this matter.

ESSAY XXIII.

Volatile mercurial salt is not the cause of the increase.

Some days after the refutation I have just mentioned, the same person sent me another of his opinions; namely, that the volatile salt is of a mercurial nature, not containing any one of the three principles absolutely pure, but mixed with the others, in

such manner that the true fixed salt exists in the volatile salt, as well as another less earthy of a sulphureous nature, and a third, greatly subtile and penetrating, partaking of the nature of mercury. Now crude and cold mercury readily penetrates through gold, and attaches itself closely to it within and without; so that it is not unreasonable to imagine, that the volatile salt of a mercurial nature, rarefied by and rendered much more penetrating by fire, should pass through the substance of the vessels, which are in like manner heated and rendered more penetrable by the fire, and should attach itself to the calx of tin, by a certain sympathy that may exist between them, as well as between gold and crude mercury. This second opinion is sufficiently overturned by the reasoning in the preceding essay; for having shewn that there can be no volatile salt in charcoal, who but must see that it cannot exist in any manner whatever? On the other hand, if this mercurial salt penetrated the vessels, it would necessarily dissolve them and make an amalgam with them, which by no means happens in our calcination. Besides, since crude mercury exhales, as we see, at a very low heat, how can it happen that this mercurial salt, being of so subtile a nature, after it has penetrated the vessel, should remain in the burning calx, without quickly flying off? Moreover, if we are to find in each of the principles all the three, I do not see why each of these again should not contain them all, and so on *ad infinitum.* These speculations are manifestly subtile, but have no foundation in nature.

ESSAY XXIV.

It is not moisture attracted by the calx that increases its weight.

Conversing, not long since, with a learned man of good judgment, I took the opportunity of mentioning this subject to him, on which, not having thought at all on the matter, he told me, that he believed that the calx, by its great dryness, attracts much moisture, and that it is this which increases its weight. I cannot approve of this opinion, for the following reasons: First, because I have never learnt that a contrary attracts its contrary; it rather flies from it, or drives it away if it can. Be-

sides, by moisture cannot be understood a naked quality, but rather water or air invested with it. As to water, whence could the calx derive it, none being near it? Is there any air more humid than usual in the laboratory in which the calcination is carried on? Will not the heat of the furnace have absorbed it? And if the calx should attract so much water, or cloudy air, as to increase its weight, a fifth part or thereabouts, as by the experiment, we should have a mortar rather than a dry calx. I add, besides, that at the instant of calcination the calx will be found increased before it can have had time to affect this imaginary attraction.

ESSAY XXV.

By one single experiment, all the opinions opposed to mine are wholly destroyed.

It is said of Hercules, that he had no sooner cut off one head of the hydra that ravaged the *Lernean marsh*, than two sprung forth—my state is like his. The error I combat abounds in opinions, which are so many heads; if I cut off one, two start up: my labour is always increasing, and I believe I shall never have done, if I employ myself in chopping them off one after another. I must collect my strength, and stiffen my arm, to sever them all at one blow, and lay the monster low in death. Let him beware, whom it concerns, for here I deal the deadly stroke. I have read in Hamerus Poppius, in the third chapter of his book, entitled "Basilica Antimonii," his new method of calcining antimony. He takes a certain quantity of it, weighs it, and, having reduced it to powder, places it in a conical form on a marble slab; then, placing a burning-glass in the sun's rays, he directs their focus on the apex of the cone of antimony, which then fumes abundantly, and in a short time the part on which the rays fall is converted into a very white calx, which he separates with a knife, and directs the rays on the remainder, till the whole becomes white, when the calcination is complete. It is a wonderful thing (he adds afterwards), that although in this calcination the antimony loses much of its substance, by the vapours and fumes which exhale copiously, yet, so it is, its

weight increases instead of diminishing. Now if we demand the cause of this increase, will Cardon tell us it is the disappearance of the celestial heat? rather it is more largely infused, by means of the solar rays. Will Scaliger say it is the consumption of its aërial parts? the particles of the calx being much attenuated and its volume increased, it rather *absorbs* them more abundantly. Will Cæsalpine allege his soot as the cause? there is no fire in this case to produce it. Can the vessel have contributed any thing towards it? the rays of the sun are so dexterously directed on the antimony, that they never touch the marble. Will any one suggest the vapours of the charcoal? none is employed. As to the volatile salts, that have been so ingeniously brought forward, they entirely lose, in this case, their savour and their grace. Peradventure moisture will be adduced, as some one very lately has thought fit to do. Whence comes it? From the marble? No, that is not conceivable. From the air? Still less so: for this operation is best performed in the hottest days of summer—in the most violent heat of the dog days, when every thing is so heated here below, that even in the shade, or during the night, the air dries wet linen, and the damp ground; and in the day-time, where the sun falls, it tans our skin, withers the grass, burns up the fruits, dries the woods, soaks up the lakes, lowers the streams of the rivers, and inflames combustible substances, as a heap of pigeons' dung. To look for humidity in the air to moisten our calx and give it weight, at that season, not by night, but by day, not in the shade but in the sunshine, not when the sun simply shines, but when its rays, collected in a concave mirror, are reflected with so much violence that they fuse and calcine metals; to look for moisture under such circumstances, I say, is to seek fire in ice, and a knot in a bulrush, as we say, a thing never to be found. Now let all the ablest minds in the world be melted into one, and this fine spirit exert itself to the very utmost of its strength, let it seek attentively on the earth and in the heavens, let it rummage all the windings and turnings of nature, yet will it not find the cause of this increase, but in the air alone, which heated, thickened and made

heavy by the sun's rays, mixes with the calx in proportion as the antimony is attenuated by the calcination, and adheres to its minutest parts. This fact fully establishes the truth of my opinion as to the increased weight of the lead and tin, which can have no other cause than the mixture of the thickened air—there being no difference between their increase and that of the antimony, except that in the latter case the air is thickened by the solar rays, and in the former by the heat of common fire.

Essay XXVI.

Why the calx does not increase in weight ad infinitum.

Having thus confuted the opposite opinions, my own alone can freely keep the field. It is true, that perceiving some objections which might disturb its progress, I step forward to set them aside. The first would seem to lead to the absurdity I have objected to Cæsalpin, that, admitting my opinion, the calx will increase *ad infinitum.* For why it will be said, will not the calx increase infinitely, since the fire may be infinitely continued, which will always supply that thickened and heavy air which causes its increase? I rid myself of this difficulty, which might insnare one of the less subtle sort, by observing, that all matter which increases by the addition of another is either solid or liquid, and that the mixture takes place between them in three ways. Either the solid matter mixes with solid, or the liquid with liquid, or one of them with the other. The mixture and increase which is effected in the two first ways has no bounds. You may mix sand with sand for ever, the increase will have no end. So you may mix wine with wine, and never have done—but it is not so with the third method, when we add and mix together a liquid with a solid: such mixed addition will not increase for ever, will not go on to infinity. Nature, in her inscrutable wisdom, has here placed bounds which she never oversteps—mix water with sand or flour, their smallest parts will be enveloped by it; pour a fresh quantity on, and they will not take up any more; and if we withdraw them from the water, they will only carry off what adheres to them, and is sufficient exactly to encompass them. Immerse them

again a hundred and a hundred times, they will not come out more loaded with it, and if left at rest in the water, they quit the superfluous quantity, and sink of their own accord to the bottom: so religiously does nature stop at the limits she has once prescribed to herself. Our calx is in this predicament; the thickened air attaches itself to it, and goes on adhering by degrees to its most minute particles: thus its weight increases from the beginning to the end; but when the whole is clothed with it, it can take no more. Do not continue your calcination with that hope; you will lose your pains. As for the rest, let not what was said in the eleventh chapter disturb you, where I used the expression, "this air, no longer air, but rather an air deprived of its nature," for these are words in excess, by which I mean only, that this air has been deprived of that liquid subtlety, which prevented its adhering to any thing, and is rendered more gross, heavy, and adhesive.

Essay XXVII.

Why all other calces and ashes do not increase in weight.

I proceed to another objection that may be started. Why do not all other calces and ashes, formed by the force of fire, increase in weight as well as the calx of tin or lead? What privilege have these more than the rest? I answer, that those things which are calcinable, or convertible into ashes, are of different natures. Some have a great deal of exhalable and evaporable matter; or (speaking like the alchymists) much sulphur and mercury, which the fire continually drives off to the end. Hence, much loss, few ashes, which cannot attach to themselves so much of the thickened air, as even to supply the loss. Others have little exhalable or evaporable matter, or in other words, little sulphur and mercury; hence small loss, much ashes (for the abundance of salt) which attracts so much of the thickened air, that not only the loss is supplied, but besides the weight increases largely beyond it. Stones, vegetables, and animals commonly are of the first order; lead and tin, of the second. There are other things whose volume is so much extended by calcination, that even if little or no matter

were lost, their weights would nevertheless greatly diminish, not so much when examined by reason, as by the balance. Such are the Indian metal called *calaem**, and some kind of saffron of Mars, as the chemists observe.

Essay XXVIII.

If Lead increase in weight like Tin.

I should have made an end, but the Sieur Brun writes to me, that having observed the increase of the tin, he made the same experiment on lead, which he found lost an ounce in the pound; which plunged him further in doubt, expecting to have found the same increase as in the tin, from the similarity of their nature, and having been calcined by the same process. But to the Sieur Brun's experiment, I oppose those of Cardan, Scaliger, and especially Cæsalpin, already brought forward, who says that it is worthy of admiration, that common lead when calcined, increases in weight eight or ten pounds per cent. Shall I array these persons in the debate, each in support of his own experiment? I am too peaceable—I reconcile them thus. One kind of lead is purer than another, whether from its coming from different mines, or from its having been melted before. The above named found an increase with the purest; the Sieur Brun a loss in the other.

Conclusion.

Behold, now the truth, whose brilliancy dazzles your eyes, which I have dragged from the deepest dungeons of obscurity, whose approach has hitherto been inaccessible. It is she who has made all the learned men sweat with vexation, who, desirous of becoming acquainted with her, have been compelled to leap over the difficulties that surrounded her. Cardan, Scaliger, Fuchsius, Cæsalpin, Libavius have sought her curiously, but never found her. Others may seek her in vain, if they follow not the road which I have first cleared for them, and made royal; all the rest being only thorny paths, and inextricable labyrinths that never lead to an end. The labour has been mine—to the reader be the profit of it, to God only the glory.—END.

* *Zinc.*

Art. VI. *On the Separation of the Proximate Principles of Animal Substances.* *By* W. T. Brande, Sec. R.S., and Prof. Chem. R.I.

In certain analyses of animal products, as for instance in ascertaining the relative proportions of albumen, gelatine and water, in the different kinds of muscular fibre, it is frequently difficult to deprive the substances under examination of humidity, without at the same time occasioning a partial decomposition of the dry residue. Under these circumstances I have availed myself with considerable advantage of the air-pump vacuum, including a surface of sulphuric acid, and have been so much struck with the rapidity and perfection of the process in these cases, as to be induced to recommend it as a very satisfactory and easy mode of depriving organic bodies in general of their adhering moisture. I have for some time contemplated a series of comparative experiments upon the varieties of muscular fibre employed as food, and one material object in these inquiries was to determine the actual quantity of nutritive matter contained in given weights of such products: as far as this has been satisfactorily effected, I shall state the results, principally as illustrating the advantage of the method above mentioned of depriving the substances of water.

500 grains of recently killed veal, free from fat, and cut into small shreds, was exposed upon a metal plate to a temperature of 212° until it ceased to lose weight, when it was found to have sustained a loss = 375 grains.

A similar portion of the same fibre, submitted to distillation in a retort at the temperature of 212°, could not be so perfectly dried, but the water which passed over had a peculiar smell, and was rendered slightly turbid by solutions of nitrate of silver, subacetate of lead, and corrosive sublimate: these solutions were rendered much more turbid by the water which passed off when the heat of the retort was elevated to 320°; so that it would appear that although rapid desiccation at 212° occasions but a slight change, exclusive of the separation of water, a partial decomposition ensues at temperatures between

212° and 320° in which the water carries over an appretiable portion of animal matter, detected not only by the smell, but by the action of the above-mentioned metallic solutions.

500 grains of the same veal were placed in a small glass basin under the receiver of an air-pump, including a circular surface of sulphuric acid of one foot diameter; during the exhaustion the meat increased in bulk from the evolution of air bubbles, but soon shrunk again, and in two hours appeared perfectly dry; it was removed, and being weighed was found to have lost 370 grains, so that the loss fell short of that occasioned by desiccation in the atmosphere at 212° by 5 grains only, or 1 per cent. These comparative experiments were frequently repeated with similar results, the desiccation at low temperatures in vacuo always proving nearly equal to that at high temperatures in the air; and being effected without any risk of decomposition, the former process was preferred to the latter.

To determine the relative proportions of gelatine and albumen in the dried fibre, it was coarsely powdered and digested in repeated portions of warm and ultimately of boiling distilled water until the residue was no longer acted upon; it was then collected upon a filter, and dried as before; the loss of weight indicated that of the matter soluble in boiling water, and which consisted almost entirely of gelatine: a very small portion of fat was sometimes observed upon some of these solutions, but where care was taken to operate upon perfectly lean muscular fibre no fat was detected in the water employed for separating the gelatinous portion.

The results of a few of these experiments are thrown together in the following table; they show the quantity of real nutritive matter contained in 100 parts of different fibres, and which, when care was taken to exclude adhering substances, was found to vary very little in the different muscles; they also show the proportion of nutritive matter soluble in water, and removed by the process of boiling at 212°.

100 parts of Muscle of	Water	Albumen or fibrine	Gelatine	Nutritive matter
Beef	74	20	6	26
Veal	75	19	6	25
Mutton	71	22	7	29

100 parts of Muscle of	Water	Albumen or fibrine	Gelatine	Nutritive matter
Pork	76	19	5	24
Chicken	73	20	7	27
Cod	79	14	7	21
Haddock	82	13	5	18
Sole	79	15	6	21

It appears evident from the above table, that although in the different kinds of meats and fish there is a slight variation in the nature and proportion of nutritive matter, it is not such as to account for the different facility with which they are digested and dissolved in the stomach: this probably depends upon some peculiarity in the solubility of the albuminous portion, and which it is my intention more particularly to examine.

The large proportion of water which is contained in muscular fibre, and the great loss of weight which it consequently suffers by desiccation I long ago observed, and was inclined to attribute it to a partial decomposition of the animal matter; but the close approximation of the results obtained by exposure to heat with those afforded by drying in vacuo, aided by the absorbent powers of sulphuric acid, show that no such decomposition occurs, and that the loss of weight sustained by muscular fibre, dried at 212°, may be referred without appretiable error, to the evaporation of water only.

Art. VII. *An Account of the Periodical Literary Journals which were published in Great Britain and Ireland, from the year* 1681, *to the middle of the Eighteenth Century. By* Samuel Parkes, F.L.S. M.R.I. *Soc. Amer. Socius., &c. &c.*

[Concluded from Page 58.]

The History of the British Periodical Literary Journals, as far as it has been written, comprises Twenty distinct articles, and brings the narrative down to the year 1720. The following is an account of the subsequent works, in the order of time in which they were published.

XXI. 1722. "Bibliotheca Literaria, being a Collection of Inscriptions, Medals, Dissertations, &c." Printed for W. and J. Innys, at the West End of Saint Paul's. London.

The editor, who was the learned Dr. Samuel Jebb, has prefixed an introduction to the first number, which I shall copy for the sake of shewing the nature of the publication.

"The undertaker of this work having observed that many things, which are useful and valuable, are in a manner lost to the world, by reason of their being too small to be separately published, has judged it will be acceptable to the learned and curious, to have 'em collected into one body, and sent abroad as occasion shall require.

"The particulars which fall within the bounds of his design are,

"I. Inscriptions and Medals, which are discovered from time to time, and cannot be printed apart, and must therefore lye concealed from the public, unless made known in this manner.

"II. The small Tracts of ancient and approved writers, which lye dispersed in libraries, and have not yet been published.

"III. Critical Dissertations upon Authors or Things.

"IV. Whatever tends to the explaining any part of Antiquity, Notes upon the Fathers or Classicks, Emendations of corrupted Passages, together with the readings of Manuscripts not already collated."

"Of these Collections he proposes to send abroad about six or seven sheets once in two months. At the close of each performance will be added an account of the labours of the learned; what works of value are preparing for the press abroad, or at home in our own universities."

The above extract will sufficiently shew the nature of this periodical work; the contributors to which were Dr. Samuel Jebb, the editor; the eminent John Masson, who was tutor in the family of Bishop Burnet; Dr. Brett, of Queen's College, Cambridge; the Rev. Joseph Wasse, rector of Aynhoe in Northamptonshire, who was the most learned man of his time;

Dr. William Wotton; Dr. Jortin; Mr. Samuel Barker, and others. The 1st and 2d numbers were published in the year 1722; the 3d, 4th, 5th, and 6th, in 1723, and the remainder in 1724. Only ten numbers of the work were published. These are usually bound in one quarto volume, consisting of about 500 pages. I consider this to be one of the most valuable Literary Journals I have; and in consequence of the opinion which the public have of it, it has long been a very scarce and dear book.

XXII. 1725. "NEW MEMOIRS OF LITERATURE." The first number of this work appeared in January 1725, and the whole was completed in six volumes 8vo. The full title runs thus, —"New Memoirs of Literature, containing an Account of New Books, printed both at home and abroad, with Dissertations upon several Subjects, Miscellaneous Observations, &c." Printed for William and John Innys. The advertisement prefixed to the first volume is short and yet sufficiently to the purpose.

"Though I had no inclination to go on with my *Memoirs of Literature*, yet I have resumed them at the desire of Messieurs Innys. I shall only observe that I have been desired to take from the Foreign Journals what I think proper to be communicated to the English readers, in order to make this new Journal more universal; and that whenever I publish an extract, of which I am only the translator, I shall give notice of it."

April 12th, 1725. MICHAEL DE LA ROCHE.

These memoirs were published monthly, without interruption, for three years. Six numbers made a volume, and each volume was concluded by a copious index. The last volume was completed at the end of the year 1727, when the work was discontinued in consequence of Messrs. Innys having been engaged by Mr. Andrew Reid to publish a new periodical Journal under the title of "The History of the Republic of Letters." In justice, however, to the memory of Mr. de la Roche, it must be said that the NEW Memoirs of Literature appear to have been

conducted with the same judgment and spirit as the former work: and that the Memoirs of Literature, and the New Memoirs, making together 14 volumes, are books which every literary man ought to possess.

The first series of eight volumes contains 597 articles, many of which are reviewed at considerable length; and not a few of them are works of great curiosity and interest. The latter six volumes (the New Memoirs) comprise 348 articles, besides the account of Books newly printed in the different countries of Europe. Each of these volumes is closed by a well arranged and copious index.

In my account of the first series of the "Memoirs of Literature," I have hinted at the valuable information which is to be derived from those volumes respecting the life and writings of Michael Servetus; it will therefore be proper to apprize the public that many other particulars respecting that great man, were afterwards collected by Mons. de la Roche, and that these Biographical Notices are to be found in the first and sixth volume of the *New* Memoirs of Literature; particularly "An Account of a very scarce edition of Ptolemy's Geography, published by Michael Servetus, in the year 1535." This edition was enriched by Servetus, with 50 maps, "each map, accompanied by an historical description, giving an account of the country and its inhabitants. These descriptions," says La Roche, "seem to be written with a good taste and judgment. The style is clear and concise, and does honour to the author." Some of the accusations, produced by Calvin against Servetus on his trial, were founded upon his publication of this work. If I am not travelling too far out of the path which I had prescribed to myself, I am desirous of observing that Servetus was likewise the editor of an edition of Pagninus's Bible, which he published at Lyons in 1542, with a preface of his own, for which the Bookseller paid him Five Hundred Livres. Both these works are now extremely rare, but the curious inquirer who may happen to be at Paris, will, perhaps, thank me for informing him that both of them may be found in the library of St. Genevieve.

XXIII. 1728. The "HISTORY OF THE PRESENT STATE OF THE REPUBLIC OF LETTERS." This very valuable work which was edited by Andrew Reid, commenced in January, 1728, and was completed in 18 vols. Octavo, in December 1736. It is upon the plan of other Journals, and contains an entertaining and instructive review of some of the best books in the English language, besides occasional papers of value from the works of foreigners. Of this class, a Translation of the Euloge upon Sir Isaac Newton, pronounced by Fontenelle before the Royal Academy of Sciences at Paris, and which is given in the 1st vol., may be taken as a favourable specimen. Each volume contains an analysis of, and extracts from, about 40 distinct publications of the time, on various branches of literature and science. The title-pages bear the following motto from Horace:

——— Fungar vice cotis, acutum
Reddére quæ ferrum valet, exsors ipsa secandi.

And from the manner in which the work was conducted, the editor appears to me to have fully redeemed the pledge which this motto continued, month after month, to offer to the public. The first volume commences with a sensible preface, highly deserving the attention of every editor of a Literary Journal; but it is too long to be transcribed for my present purpose. The motto which the author has chosen for this preface is

"Tros Rutulusve fuat, nullo discrimine habebo."

A work of this extent, and containing such variety of matter, ought to have had a general index. In this it is deficient; but each number contains a table of contents, and each volume closes with a very complete index to that particular volume. It is not very often that a complete set of the "Present State of the Republick of Letters" is offered for sale, but the work is certainly not so scarce as some of the more early literary journals; and detached volumes are so often to be met with, that an industrious collector might perfect a set without much difficulty, and at a trifling expense.

XXIV. 1730. "A LITERARY JOURNAL, *or a Continuation of the Memoirs of Literature.* By the same Author." (Meaning Mons. de la Roche.) The first volume commences in January, and concludes with the number for June, 1730. Vol. II. from July to December, 1730, octavo. They were sold by Knaplock, at the Bishop's Head, in St. Paul's Church Yard.

On the back of the title-page of the last number of the New Memoirs of Literature, M. de la Roche announced that he should discontinue writing those Memoirs; and on the same page he says, "When I publish another English Journal with another title, I shall give notice of it."

Accordingly, two years afterwards, this learned and indefatigable man began a new work, under the title which stands at the head of this article, and from the attention which I have been able to pay to this performance, it appears to me to have been conducted with his usual care and discrimination. I have seen only the second volume, but I can pronounce that this abounds with curious matter, as well as a regular review of many very important works. This Journal was published quarterly, and at the back of the table of contents of the number for the last quarter of the year, the following advertisement appears:

"This part of the Journal would have come out at Christmass if I had not been sick. I shall always use my utmost endeavors to publish these Papers in due time. As I print this work upon my own account, I hope my Readers will do me the favour to recommend it to their friends." That M. de la Roche printed this work *on his own account*, may be readily believed, as it was got up in a style far superior to any English Journal that had then appeared. The paper is fine, the types are very similar to Baskerville's, and the manner in which it is printed would not disgrace the proudest Printer in our Metropolis at the present day.

XXV. 1730. "HISTORIA LITTERARIA," or an exact and

early account of the most valuable books published in the several parts of Europe, with a complete alphabetical index."

> "*Floriferis ut apes in saltibus omnia libant,*
> *Omnia nos itidem.*——————Lucret.

London, Printed for N. Prevost, over against Southampton street, in the Strand. 1731.

This Journal, which was published in monthly numbers at one shilling each, commenced in the year 1730, and closed in 1733, or early in the year 1734, forming four volumes in octavo. It was written by the noted Archibald Bower, formerly Professor of Rhetoric, History, and Philosophy, in the Universities of Rome, Fermo, and Macerata, and author of the History of the Popes, in seven volumes, quarto. It was undertaken and completed by him while he resided with Lord Aylmer. With this nobleman he lived on terms of the greatest intimacy, and was introduced by him to a large circle of literary acquaintance.

It is a curious circumstance that he (Archibald Bower) wrote the preface, and several of the first articles, in Italian, because he did not sufficiently understand English to allow him to write in our language; but finding the inconvenience and expense of employing a translator, he applied so closely to the study of the English tongue, that in six months he was enabled to dismiss his translators *, and continue the work himself in English †.

The following extracts from the Preface will shew the plan and design of the work:

I. "We propose to take notice of none but the most valuable books, and such as are *last* published, and have not been mentioned by any other of our Journalists. Of these, whether Latin, Italian, French, English, *&c.*, we shall give faithful extracts. The choice and novelty of books is what chiefly recommends a Journal; and the correspondents we have already settled is such, that we may confidently affirm, no work of any figure or

* The several articles which he wrote in Italian were translated by Mr. Barkley, who was the master of an academy of some note at Little Chelsea; and the preface was translated by Mr. Lockman.

† Chalmers, vol. VI. page 257.

reputation, will be published in any part of Europe, but we shall immediately give an account of it. The extracts we shall make will enable such persons as have not much time upon their hands, to treasure up a great number of excellent observa tions, in the various branches of literature; and at the same time acquaint them with whatever is worthy observation in the works of those writers who are the ornament of the age."

II. " Our Journal will include all subjects; nor will Dissertations on Medals, Inscriptions, and other valuable remains of antiquity be omitted."

" This Journal will always consist of Five sheets and a half, and be published regularly at the beginning of every month, Six numbers will make a volume, to which will be annexed a Catalogue of the Authors from whom the Extracts are made, together with an Index. At the end of each Journal will be inserted the freshest accounts of all Works just published, and of all Old Authors reprinted in any part of Europe, as soon as our Correspondents shall transmit us such accounts. And lastly, we shall add a Catalogue of New Books imported monthly."

As an Editor, Mr. Bower was indefatigable; and the manner in which he executed the HISTORIA LITERARIA exalted his reputation considerably. His intimacy with Lord Lyttelton and other persons of eminent learning, together with his residence in the family of Lord Aylmer, occasioned him to be still more known to the public; and while busily employed in conducting the Historia Literaria the Proprietors of the Universal History applied to him to engage him in that undertaking *. Some very advantageous offers were made to him, but so attached was he to the employment in which he was then occupied and so desirous to render the Historia Literaria deserving of the patronage of the public, that he declined listening to their proposals until the year 1734, when the publication of the Historia Literaria was relinquished. It may be added that in the following year, Mr. Bower did agree with the proprietors of the Universal

* Chalmers, Vol. VI. page 253.

History, and continued to be one of the editors of that voluminous work, for nine years afterwards.

XXVI. 1733. "THE BEE, or Universal WEEKLY Pamphlet, by a Society of Gentlemen and Booksellers, containing, among other things, an account of the most remarkable Books published abroad, and a catalogue of all books and pamphlets published at home in the week, with short reflexions upon such as deserve it." Each number consists of 44 pages, price 6d, and the Title page of each contains a wood-cut of a Bee-hive in the midst of a huge heap of books of all kinds and sizes. This work, which is complete in 7 Vols. Octavo, was planned and conducted by the unfortunate Eustace Budgell, son of Dr. Gilbert Budgell, and grandson of Dr. Gulston, Bishop of Bristol. This elegant author was early in life engaged with Addison, his intimate friend and relative, in writing papers for the Spectator and Guardian. Those marked with an asterisk were written by Mr. Budgell. The first weekly number of the Bee was published in February 1733, and the last number on the 30th November 1734. Thirteen numbers formed a volume, so that a volume was completed every three months. The life of the editor of this work was chequered with various adventures, and his death was of the most unfortunate and melancholy kind. A particular account of his history may be seen in Chalmers, Vol. VII. page 246—251.

XXVII. 1735. "THE LITERARY MAGAZINE, or the History of the Works of the Learned, containing an account of the most valuable Books published both at Home and Abroad, in most of the Languages in Europe, and in all Arts and Sciences; with proper Observations on each Author. To which are occasionally added, Biographical Memoirs, Dissertations, and Critical Enquiries. By a Society of Gentlemen." The title of this Journal which I have copied entire, will be sufficient to shew its nature and objects. One of the principal editors was Ephraim Chambers, the Author of the English Cyclopædia in 2 vols. folio. The Literary Magazine was published monthly in

Octavo Numbers, commencing in January 1735, and 12 numbers formed a volume. The second volume ends with December 1736; and I have reason to think more volumes were published. There are two Indexes to each of the volumes I have; the first being a list of books reviewed; the last an alphabetical reference to all the miscellaneous matter contained in it.

In the preface, Mr. Chambers carries the antiquity of Literary Journals much higher than any writer with whom I am acquainted.

" A history of the works of the Learned," says he, " is far from being a new project. Apollodorus an Athenian, who lived about two hundred and forty years before the Nativity of Christ, composed a work of this sort, which he called, *a Library of the Origin of the Gods;* that is, a collection of the most ancient histories, as they lie disguised under fables and fictions. We have still three books of it. Diodorus of Sicily, in the reign of Augustus, spent about thirty years in composing an *Historical Library*, in forty books, of which fifteen are now extant. But the richest and most comprehensive work of this sort is the *Myriobiblia*, written by *Photius*, patriarch of Constantinople, in the middle of the ninth century of the christian æra. It is usually called the Library of Photius, and contains the arguments or extracts of nearly two hundred and eighty volumes of different authors, on several subjects *. These laborious and useful works have preserved to us some valuable fragments of antiquity, which would otherwise never have come to our knowledge."

* This Bibliotheca was composed by Photius while he was an Ambassador in Assyria. It seems our editor did not overrate this work, for Fabricius calls it, " *non liber sed insignis thesaurus*, not a book, but an illustrious treasure," in which are contained many curious things no where else to be found. David Hoeschelius first caused it to be printed in 1601; in Greek, at Vienna. Schottus translated it into Latin, and afterwards the Greek text and the translation were printed together at Geneva in 1611, but the best edition is that printed at Rouen in 1653, folio, under the title " Photii Myriobiblion," *&c.* There are large-paper-copies of this edition, which bear a very high price. See Chalmers Vol. XXIV. page 473.

In the conclusion of the preface we find the following judicious observations. " The spirit of partiality is a dangerous rock, on which many Journalists have been lost. We conceive it to be the duty of a Journalist to give a faithful account of the books which come into his hands. When he affects the air and language of a censor, he invades the undoubted right of the Public, which is the only sovereign judge of the reputation of an Author, and the merit of his compositions. To the same judge we must submit the present performance; with a fix'd resolution of neither offending those from whom we may chance to differ in opinion, nor misrepresenting, nor disguising the sentiments of authors, when contrary to our own. We shall be particularly careful to insert nothing, which has not some tendency to improve the mind, form the judgment, or entertain the reasonable and commendable curiosity, of our readers."

XXVIII. 1737. The " BRITISH LIBRARIAN: Exhibiting a compendious Review or Abstract of our most scarce, useful, and valuable Books in all Sciences, as well in Manuscript as in Print: with many Characters, Historical and Critical, of the Authors, their Antagonists, *&c.* In a manner never before attempted, and useful to all Readers. With a complete Index to the volume. *Multa renascentur quæ jam cecidere.*" London, printed for T. Osborne, in Gray's-Inn, 1738.

Although this was an anonymous publication it is well known that the Author of it was the celebrated Bibliographer William Oldys, Librarian to Robert Harley, Earl of Oxford, and one of the compilers of the Harleian Miscellany. As only six numbers of this Journal were ever published, the work is complete in one volume. For several years, this curious Journal was entirely neglected by the Book Collectors, but it is now valued as it deserves. It is a thin volume of 418 pages, and in Triphook's Catalogue for 1816 it stands valued at a Guinea, but since that time a copy was sold at Mr. Evans's sale in Pall-Mall for 1*l.* 11*s.* 6*d.*

It is observable that the BRITISH LIBRARIAN is chiefly a review of very scarce old books, and it is highly valuable because

it contains many curious details from interesting books, which are no where else at this time to be found. The situation which Mr. Oldys held with Lord Oxford gave him the best opportunity for gratifying his taste for ancient and curious books and manuscripts, and hi industry was such, that he made the most of his advantages. He is well known to have been a man of the strictest integrity—therefore whatever he asserts as of his own knowledge, in the British Librarian or in his other works, may be depended upon. An instance of his probity occurred soon after the publication of his Life of Sir Walter Raleigh, that was prefixed to the Folio edition of Sir Walter's History of the world. Some Booksellers thinking his name would tend to sell a work they were publishing, offered him a considerable sum of money if he would allow them to affix it—but he rejected the proposal with the greatest indignation, though at the time he was in great pecuniary distress.

Mr. Oldys was a writer on a great variety of subjects, and several of his MS. volumes are preserved in the British Museum. Mr. Chalmers has made out the most complete list of his works that he was able to compile, amounting to 24 articles*—from whence it appears that he was the author of several Lives in the Biographia Britannica, bearing the signature G. the initial letter of Gray's-Inn, where he formerly lived. He was also a writer in the General Dictionary, and furnished several communications to Mrs. Cooper, for that curious volume which she published under the title of the "Muses Library." A translation of Camden's Britannia, in two volumes quarto, has also been ascribed to him by Mr. Gough; and among the Birch MSS. in the British Museum, are some Memoirs of the family of Oldys, drawn up by William Oldys himself.

The following well-turned anagram was found in one of his MSS.

W. O.

In word and Will I AM a friend to you,
And one friend OLD IS worth a hundred new.

I should have been glad to give a list of the books which Mr.

* General Biog. Dict. Vol. XXIII. p. 336.

Oldys has reviewed in the British Librarian, as it could not fail to create a desire in many readers to possess the volume, but I thought the catalogue might be deemed too long for the Journal of Science, Literature, and the Arts. There are few books whose value might not be enhanced by a good Index: the British Librarian, which requires such an appendage as much as most books, is terminated by an alphabetical Index of 52 columns closely printed.

XXIX. 1737. "THE HISTORY OF THE WORKS OF THE LEARNED, giving a general view of the state of Learning throughout Europe, and containing an impartial account and accurate Abstracts of the most valuable Books published in Great Britain and Foreign parts."

"Interspersed with Dissertations on several curious and entertaining subjects, critical Reflections, and Memoirs of the most eminent Writers in all branches of polite Literature. London, printed for T. Cooper, at the Globe in Paternoster-Row, *&c.* 1737."

This work commenced in January 1737, and was regularly published in Monthly Numbers, six of which formed a volume; but there was no regular series of progressive numbers, as is usual in works of this nature, the volumes in each year being marked I and II. This Journal met with so much encouragement that it was published without intermission until the end of the year 1743, when the last Number was printed, which completed the fourteenth volume. The plan of the Editor is sufficiently explained in a copious Preface prefixed to the first volume, but I have not been able to ascertain by whom the work was written or edited. Each volume contains a review of about 40 or 50 distinct articles, and an alphabetical Index to every six numbers adds very much to the value of the work.

XXX. 1744. "A LITERARY JOURNAL." The following is a copy of the Title pages of the first Volume. "A Literary Journal for October, November, December, 1744. Vol. I. Part I. Dublin, Printed by S. Powell, for the Author, 1744." "A

Literary Journal for January, February, March, 1745. Vol. I. Part II. Dublin printed by S. Powell for the Author 1745." As this is the only Literary Journal that I am acquainted with, which was published in Ireland, during the whole of the period from the printing of the first British Journal in the year 1681 to the middle of the last Century; and as the work has for many years been very rare in England, I conceive that those persons who can read this article with any degree of interest, will be glad to have a more particular account of this Periodical Dublin Journal, than has been given of some of the works that preceded it.

The principal design of the Editor of the Dublin Journal was to give an account of the best Foreign books, published in the various languages of Europe; with such extracts from each as were most likely to interest the English reader. It commenced at the latter end of the year 1744, and was intended to be published quarterly, at the price of one shilling and sixpence for each part. The work is now complete in Five volumes Octavo, two Quarterly Numbers forming each of the three first volumes, as is the case with the "Quarterly Journal of Science, Literature and the Arts," now edited at the Royal Institution of Great Britain; but on account of a delay in the publication which was occasioned by the ill state of health of the editor, the fourth volume is made to consist of two half yearly numbers from March 1746 to March 1747; and in consequence of continued illness the 1st part of Vol. V. extends from March 1747 to March 1748, and the last number includes the literature of Europe from March 1748 to the middle of the year 1749.

From the manner in which the editor of the Dublin Journal executed his task, the work must, as I conceive, have fully answered the expectations of those literary persons who had patronised the undertaking; but whether the number of general readers in Ireland was insufficient in the middle of the eighteenth century to support such a periodical work, or whether it was owing to the continued ill health of the editor, I know not; but it appears that the publication was entirely discontinued in the month of June 1749, which was only one month after the

commencement of our great national work, the "Monthly Review."

It is observable that the "History of the works of the Learned" (the octavo series) was brought down to the end of the year 1743; and that the Irish Journal was begun in 1744, and carried on until the Monthly Review commenced in the year 1749. Thus the chasm which there would otherwise have been in the series of British Journals, is filled up, and we have now almost a complete succession of works of this nature in the English language from the year 1681 until the present day.

In consequence of the entertainment and instruction which I have myself derived from the perusal of the Dublin Journal, I am desirous of giving some farther particulars respecting the plan of the work, and the manner in which it was executed.

As to the plan of the publication, that will perhaps best appear from the words of the Author in the preface to the first volume. "As Foreign books," says he, "are only known from the French Journals, understood by few, and read by fewer, my intention is to give *English* abstracts of the most important foreign books, German, Dutch, French, or Latin. To execute this scheme, I shall chuse the best Abstracts to be found in the great variety of foreign Journals; enlarge upon what shall be judged to be of the greatest moment; and suppress what shall appear to be of small use. I shall also venture some short remarks of my own, when necessary to the better understanding of the subject; and sometimes give abstracts not to be met with in any Journal."

"Tho' my principal design is to give information of Foreign books, yet I do not mean so to confine myself as never to take notice of English writers, who treat of matters either entirely new, or remarkably curious; but satyr and personal reflections, shall be totally excluded from these papers."

"The favourable reception of this undertaking must necessarily depend on the execution. Success will encourage me to go on, and to give four parts, Octavo, every year, one each quarter, containing about fourteen sheets, at the rate of one shilling and sixpence English money, each part."

"All books of note published abroad, of which no abstract is given, shall be exactly mentioned at the end of each volume, with whatever happens remarkable in the Universities of *Muscovy*, *Sweden*, *Denmark*, *Germany*, *Holland*, *Switzerland*, *and France*."

"A Table of Contents, and an Index of Authors, shall be printed with each volume. My correspondents are desired to frank their Letters, and direct them to the Reverend Mr. Droz in College Green, Dublin*."

Having thus stated the design of the author, justice to his memory obliges me to say that he has produced a very interesting work, abounding with curious and useful matter; and that the various observations and reflections which are interspersed throughout the five volumes, bespeak a man of education and discernment. Each Number contains a Review of from 10 to 14 distinct articles, which are followed by a chapter of Literary News from all the principal Cities and Towns of Europe. This Chapter of Literary News is very comprehensive,, as it contains not only a Quarterly account of valuable books published in different parts of Europe; but anecdotes of learned men; an account of the formation of New printing establishments; the erection of New Academies; the founding of Literary Societies; the establishment of public Libraries; an account of New works in the press; the present employment and future engagements of men of letters; an analysis of the Memoirs of Learned Societies; and an announcement of the Death of eminent men, with an account of their principal writings, *&c. &c.* The chapter concludes with an account of the new books published during the last Quarter, in Great Britain and Ireland. A Table of contents, and a General Index closes each volume.

XXXI. 1746. "THE MUSEUM, or the Literary and Historical Register." London: Printed for R. Dodsley, in Pall-

* During the publication of this Literary Journal, Mr. J. P. Droz was an eminent Bookseller on College Green, Dublin; but whether he was related to the Rev. Mr. Droz of College Green, the learned editor of this work, I have not been able to ascertain.

mall, 1746." The name of Dodsley, at first sight, prejudiced me very much in favour of this Journal. The intention of the ingenious and accomplished Editor of this work appears to have been to unite the character of a Periodical Review of Books with that of a Literary Magazine; and in my judgment he has completely succeeded in making his volumes very entertaining and at the same time highly interesting and useful.

The work was regularly published in Numbers, one number, consisting of 40 pages, every Saturday fortnight, and thirteen such numbers formed a volume in Octavo, three of which completed the work. Although Mr. Dodsley projected this periodical publication, only one fourth share of it belonged to him, the remainder being the property of Messrs. Longman, Rivington and others.

The work consists of Literary and Moral Essays; some original poetry; Literary Memoirs, including a Review of Books; and historical memoirs, including a detailed account of the Rebellion which had just been suppressed in these kingdoms. The real nature of the work will, however, be better understood by the perusal of a few abstracts from the preface which was given with the last number of the first volume.

" The favourable reception this work has met with from those whom it is an honour to have pleased, not only calls for acknowledgment, but demands our utmost endeavours to deserve the continuance of their Patronage and Assistance. In our account of Books, we hope no author has been misrepresented, no reader justly disgusted, or greatly disappointed; and we shall endeavour to let no considerable work escape our notice."

" We take this opportunity of returning thanks to those who have favoured us with their assistance in this undertaking; and we hope, by degrees, the Learned and Ingenious, in all Arts and Sciences, will honour us with their correspondence; and by that means the Museum may become a general vehicle by which the Literati of the whole kingdom may converse with each other, and communicate their knowledge to the world. How agreeable such an intercourse must be, and how highly condu-

cive to the advancement of Literature, we need not say. We therefore flatter ourselves, that such as have any useful knowledge to communicate—or any hint that may contribute to improve the mind, polish the manners, refine the taste, or mend the heart, will be as glad of such an opportunity of communicating it, as we shall be always proud of conveying it to the Public."

So much for the language, opinions and promises of the Editor. A modern writer (Mr. Chalmers) speaking of this Journal says, "It extended to three volumes, and contains a greater "variety of original Essays of real merit, than any similar un- "dertaking—nor will this be doubted, when it is added that "among the contributors were Spence,—Horace Walpole,—the "two Wartons—Akenside—Lowth—Smart—Gilbert Cooper,— "Whitehead—Merrick—and Campbell."

Each volume contains some considerable Indexes divided into classes, thus. Index to the Essays. Index to the Poetry. Index to the account of Books. Index to the History of the present state of Europe. Index of Names to the whole. The 1st Volume has also an Index to the history of the Rebellion.

XXXII. 1749. "The Monthly Review, A Periodical Work, giving an account, with proper abstracts of, and Extracts from, the New Books, Pamphlets, *&c.*, as they come out. By several Hands. London, Printed for R. Griffiths, at the Dunciad in St. Paul's Church Yard." The first Number of this work, for May 1749, was published on the 1st day of the following month, without any Preface or Advertisement whatever. And though the editor was entirely without patronage or support, he persevered in printing the work, month after month, and continued to publish it in the most regular manner possible, for more than half a century.

A few of the early numbers were sold at sixpence, and the others at one shilling each. It having, however, been found inconvenient to delay the publication of the last number of each volume, for the sake of having sufficient time to prepare the General Index, Mr., afterwards Doctor, Griffiths announced, in the Number for

June 1753, that on the 15th July would be published, price six pence, an Appendix to the eighth volume, and that this should contain, in three sheets, the General Title, Index's, *&c.* I observe, however, that it was not until the publication of the Appendix to the 28th volume, printed in 1763, that the editor informed the public that in future he should reserve the Appendix for the review of foreign books exclusively. This plan must surely have given general satisfaction, as it has been adhered to ever since. From this time, the Monthly Review was published with the utmost punctuality; seven numbers, together with a Table of Contents, and an Index to remarkable passages, forming a volume. In consequence, however, of the increased number of books issuing from the British and foreign presses, the Proprietor at length determined that it would be advisable to alter the plan of publication; and for the future to give the work to the public in numbers of a larger size, and at an advanced price; four numbers with an Appendix, forming a volume, and three volumes instead of two, being thus annually completed. This alteration commenced with the beginning of the year 1790, and the whole of the work since published, is known by the name of the "New Series of the Monthly Review." The former Numbers of the Review, now known by the name of the "Old Series," were published with the design of forming two volumes in each year; these come down to the end of the year 1789, and make in the whole 81 volumes.

In the year 1786 Mr. Griffiths published in two volumes a General Index to the first seventy volumes of the Monthly Review, compiled by the Rev. S. Ayscough, of the British Museum. The 1st volume of this Index contains a "Catalogue of the Books and Pamphlets characterized; with the size and price of each article; to which is added an Index of the Names mentioned in the Catalogue." The second volume contains "An Alphabetical Index to all the memorable Passages, with Literary Anecdotes, Critical Remarks, *&c. &c.* contained in the Monthly Review during that period." Ten years after this Index was printed, Mr. Griffiths published a third Volume of Index to the Monthly Review, commencing with the seventy-

first and ending with the eighty-first volume. This third volume is compiled on the same plan as the former, and by the same Gentleman. The eighty-one volumes from the year 1749 to the end of the year 1789, together with the three volumes of Indexes, form a complete and highly useful set of books which are now known by the name of the first series of the Monthly Review.

The New Series commenced in the Month of January 1790. The first four months of the year, and an Appendix, formed the first volume; the title of which is as follows. "The Monthly Review; or Literary Journal, enlarged:——From January to April inclusive, 1790. With an Appendix."

> "—— Major rerum mihi nascitur ordo,
> Majus opus moveo." *Virg. Æn.* VII. 44.

> "But you who seek to give and merit fame,
> And justly bear a Critic's noble name—
> Fear not the anger of the wise to raise;
> Those best can bear reproof, who merit praise."

"London: Printed for R. Griffiths; and sold by T. Becket' in Pall-Mall. 1790."

The New series of the Monthly Review, altho' it has been indebted to a long line of writers of various talents and acquirements for the character which it has supported and for the rank which it now holds among the literary productions of the present day, seems at no time to have forfeited the good opinion of the public; for it has been regularly published every month, without intermission; and has very ably supported its credit amidst numerous competitors. In the month of May last, the 97th volume of this series was completed. Both the Series therefore now form 178 volumes, besides the General Indexes.

It may here be remarked that while Mr. Griffiths was engaged in forming the plan of the Monthly Review, the learned and accomplished Dr. Matthew Maty *, who was afterwards ap-

* Dr. Matthew Maty was the Father of the Rev. Paul Henry Maty, the author of a learned Journal in 10 volumes Octavo which commenced in 1782 under the Title of the "New Review with Literary Curiosities and

pointed Secretary to the Royal Society and principal Librarian at the British Museum, was employed in bringing out a similar work in the French Language, under the title of the "Journal Britannique." This work which commenced in January 1750 was completed in 18 very small volumes, the last of which appeared in December 1755 *. A continuation was then undertaken by Mr. De Mauve, and this was carried on to the extent of six volumes more, until the end of the year 1757.

Another respectable work was also planned about this time, entitled "The Universal Librarian," the first Number of which purports to be a Review of Books for the Months of April, May and June, 1751, price 3s. : but as I never proposed to proceed farther in my inquiries respecting the Periodical Literary Journals than to the establishment of the Monthly Review, I must hasten to conclude my account of that important work.

The editor of the Monthly Review was Mr. Ralph Griffiths, who was one of the sons of a Supervisor of his Majesties Excise in the County of Salop; but who for the sake of improvement and to gratify his taste for reading, left the country, and placed himself with a Bookseller in the Metropolis. Here, by care and frugality, he saved sufficient to enable him to begin the book-selling business on his own account; and accordingly, in the year 1747, he opened a shop in Saint Paul's Church Yard and took the sign of the Dunciad †. Here this extraordinary man formed the plan of the Monthly Review, and in two years

Literary Intelligence." In reference to his Father having been the Editor of the *Journal Britannique*, he adopted for his work the following pleasing motto;

"*Sequitur patrem, non passibus æquis.*"

* The Journal Britannique was held in high estimation by Gibbon the Historian. See Gibbon's Memoirs in Quarto Vol I. page 87.

† In 1754 Mr. Griffiths removed to the Dunciad in Paternoster Row, and in 1759 to the Dunciad, near Catherine-street in the Strand; where his shop was the favourite lounge of Dr. Goldsmith and other literary characters of the day. In the year 1764 the Name of Mr. Becket appeared for the first time, in the title-page as the publisher of the Monthly Review; Mr. Griffiths having relinquished business, and quitted London, to attend solely to his Literary Journal.

after his settlement in Saint Paul's Church yard the publication of the work commenced, as already related.

It is remarkable that Mr. Griffiths not only planned this important Work, but that he gave it to the world without any recommendatory Preface, or any encomiastic explanation whatever of the scheme of the undertaking; that he continued the publication without private patronage of any kind; and actually lived to witness its growing prosperity, and to superintend and conduct the press for fifty-four years. The profits arising from the sale of this very respectable and useful Journal, enabled him to live in a state of comfort, and latterly of opulence.—His Son, George Edward Griffiths, Esq. of Turnham Green, has continued to be the Editor ever since the demise of his Father.

It has already been observed that the contributors to the Monthly Review were Gentlemen of the most respectable characters; and I have understood that the first article in the work was written by Dr. William Rose, who kept an academy of great respectability at Chiswick, and was author of a well known translation of Sallust*. This Gentleman was brother-in-law to Mr. (afterwards Dr.) Griffiths and also to the Rev. Jabez Hirons of St. Albans; these three gentlemen having married the three daughters of the Rev. Dr. Samuel Clarke of the latter place. Another constant contributor was the Rev. William Ludlam the Mathematician, author of many esteemed works on astronomy and mathematics, Rector of Cuckfield in Sussex, and Vicar of Norton by Galby, Fellow of John's College, Cambridge, *&c. &c.* The Rev. Thomas Gwatkin, who was afterwards Professor of Mathematics and Natural Phi-

* The literary rank which Dr. Rose held with his contemporaries, may be readily conceived from the following circumstances. He was the constant friend and counsellor of Andrew Miller, the eminent Bookseller in the Strand; Dr. Lysons speaks in the handsomest way of him in his History, and Arthur Murphy wrote lines upon his death, which are inscribed upon his tomb. A further account of this excellent man, may be seen in Nichols's Anecdotes of the 18th Century, Vol. III. p. 386.

† Nicholson's Literary Anecdotes, Vol. III. page 506.

losophy in the College of William and Mary in the State of Virginia, was also an occasional writer for the work; but one of the most constant contributors was the Rev. Samuel Badcock, a respectable Dissenting Minister, who is said to have been the author of those elegant Sermons which were preached in the year 1784 at the Bampton Lectures, delivered before the University of Oxford, by Dr. Joseph White, Professor of Arabic in that University. This Gentleman, as I have understood, was connected with the Proprietor of the Monthly Review for many years.

Dr. Griffiths died at Turnham Green on the 28th September 1803, in the 83d year of his age, and Mrs. Griffiths on the 24th August 1812 ‡.

I have already said that Dr. Griffiths published two General Indexes to the First Series of the Monthly Review, consisting of 81 volumes. In the year 1818 an Index to the first Eighty-one volumes of the *New* Series was also published. This latter was compiled by Mr. J. Cudlipp, in two thick volumes, and is sold at two guineas and a half in boards. The title to this Index runs thus: " A General Index to the Monthly Review, from the Commencement of the New Series in January 1790 to the end of the Eighty-first volume completed in December 1816. In Two Volumes. London, Sold by John Porter, Pall Mall, 1818."

The Public have thus a very valuable and connected Series of Literary Journals, extending to no less than 162 volumes *, besides Five volumes of General Indexes.

A work of this magnitude, embracing a period of more than seventy years, would be an unwieldy incumbrance, and comparatively of little use were it not for the volumes of General Indexes already referred to, and which may be now had to accompany it—but with this accompaniment a Library cannot contain a set of books of more value, nor of more general utility.

Having been a regular reader of the Monthly Review for more

* As the work is still in the course of publication, 16 volumes more have been completed, but these at present have no General Index.

than Forty years, I apprehend I can form a tolerably correct opinion of the general merits of the work and of the manner in which it has usually been conducted—and I do conceive that I am justified in saying that I know of no publication in the English Language, that is better calculated to give young persons a general knowledge of men and of books, provided they acquire the habit of frequently consulting it by means of the General Indexes.

Although I never had any intercourse with the late Dr. Griffiths, nor have any personal knowledge of the present Editor of the work, I feel pleasure in availing myself of the opportunity which this History of the British Literary Journals affords me of expressing my approbation of a Book which I consider in the light of an old acquaintance from whom I have derived much valuable information, and to whom I am under no small degree of obligation.

Mecklenburgh-square,
March 20*th*, 1822.

SAMUEL PARKES.

ART. VIII. *On Prussic Acid. By* ANDREW URE, M.D., F.R.S., *&c.*

HAVING been frequently consulted by physicians and apothecaries concerning the strength of the dilute prussic or hydrocyanic acid employed in medicine, I instituted a series of experiments, to determine the relation between its specific gravity and quantity of real acid. The acid which I prepared with this view, had a specific gravity = 0.957.

The following table comprehends their results.

Quantity of above liquid Acid.	Specific Gravity.	Real Acid pr. Ct.
100.0	0.9570	16
66.6	0.9768	10.6
57.0	0.9815	9.1
50.0	0.9840	8.0
44.4	0.9870	7.3
40.0	0.9890	6.4
36.4	0.9900	5.8

Quantity of above liquid Acid.	Specific Gravity.	Real Acid p. Ct.
33.3	0.9914	5.3
30.8	0.9923	5.0
28.6	0.9930	4.6
25.0	0.9940	4.0
22.2	0.9945	3.6
20.0	0.9952	3.2
18.2	0.9958	3.0
16.6	0.9964	2.7
15.4	0.9967	2.5
14.3	0.9970	2.3
13.3	0.9973	2.1
12.5	0.9974	2.0
11.8	0.9975	1.77
10.5	0.9978	1.68
10.0	0.9979	1.60

From the preceding table, it is obvious that for acid of specific gravity 0.996 or 0.997, such as is usually prescribed in Medicine, the density is a criterion of greater nicety than can be conveniently used by the majority of practitioners. In fact, the liquid at 0.996 contains about double the quantity of real acid, which it does at 0.998. It is therefore desirable to have another test of the strength of this powerful and dangerous medicine, which shall be easier in use, and more delicate in its indications. Such a test is afforded by the red oxide of mercury, the common red precipitate of the shops. The prime equivalent of prussic acid, is exactly one-eighth of that of the mercurial peroxide. But as the prussiate of mercury consists of two primes of acid to one of base, or is in its dry crystalline state, a bicyanide, we have the relation of one to four in the formation of that salt, when we act on the peroxide with cold prussic acid. Hence we derive the following simple rule of analysis. To 100 grains, or any other convenient quantity of the acid, contained in a small phial, add in succession, small quantities of the peroxide of mercury in fine powder, till it ceases to be dissolved on agitation. The weight of the red precipitate taken up being divided by four, gives a quotient representing the quantity of real prus-

sic acid present. By weighing out beforehand, on a piece of paper, or a watch glass, forty or fifty grains of the peroxide, the residual weight of it shews at once the quantity expended.

The operation may be always completed in five minutes, for the red precipitate dissolves as rapidly in the dilute prussic acid, with the aid of slight agitation, as sugar dissolves in water. Should the presence of muriatic acid be suspected, then the specific gravity of the liquid being compared with the numbers in the above table, and with the weight of peroxide dissolved, will shew how far the suspicion is well founded. Thus, if 100 grains of acid, specific gravity 0.996, dissolve more than 12 grains of the red precipitate, we may be sure that the liquid has been contaminated with muriatic acid. Nitrate of silver, in common cases so valuable a re-agent for muriatic acid, is unfortunately of little use here; for it gives with prussic acid, a flocculent white precipitate, soluble in water of ammonia, and insoluble in nitric acid, which may be easily mistaken by common observers, for the chloride of that metal. But the difference in the volatility of prussiate and muriate of ammonia, may be had recourse to with advantage; the former exhaling at a very gentle heat, the latter requiring a subliming temperature of about 300° Fahrenheit. After adding ammonia in slight excess to the prussic acid, if we evaporate to dryness at a heat of 212°, we may infer from the residuary sal-ammoniac, the quantity of muriatic acid present.

The preceding table is the result of experiments which I made some time ago at Glasgow. I have lately verified its accuracy by experiments made at the Apothecaries' Hall, London, on their pure prussic acid. 100 grains of the bi-cyanide of mercury, require for their conversion into bichloride (corrosive sublimate), 28.56 grains of chlorine, a quantity to be found in 100 grains of muriatic acid, specific gravity 1.1452. And as 100 grains of the bi-cyanide contain 20.6 of real prussic acid, they will furnish, by careful distillation on a water-bath, a quantity of liquid acid, equivalent to 700 grains of the medicinal strength 0.996. By consulting my table of muriatic acid, published in this Journal for January last, the quantity of it at any

density, necessary for decomposing the above cyanide, will be immediately found; bearing in mind, that 32.5 = the prime equivalent of salt, corresponds to 9 of chlorine.

In Dr. Thomson's System of Chemistry, it is said, that the red precipitate of mercury, is " somewhat soluble in water *." This statement, were it true, would militate against the use of that substance, as a test of prussic acid. But on digesting water with heat, on the mercurial peroxide, and passing a current of sulphuretted hydrogen through the filtered liquid, I could find no traces of mercury.

Art. IX. *On the Changing of Vegetable Colours as an Alkaline Property, and on some Bodies possessing it.* By M. Faraday, Chem. Assist., Royal Institution.

The changes produced by acids and alkalies on vegetable colours, have long been considered as very distinctive and peculiar effects, and even sufficient of themselves to indicate the presence of these bodies. Since the introduction into those classes of substances before excluded, as of silica, various oxides, and vegetable substances, into the class of acids; and of oxide of lead, morphia, &c., into the class of alkalies; it becomes more important to substantiate any particular property as peculiar to those classes, or show its fallacy, by pointing out to what substances excluded from them it also belongs.

At present I shall detail the results of a few experiments made on the colouring matter of turmeric and rhubarb, comparing the changes produced on them by alkalies to those occasioned by some other bodies. At Vol. V. p. 125, I mentioned the property possessed by muriatic acid gas and strong acids in general, of reddening or browning turmeric paper. I find that in general they have the same effect on rhubarb paper, and a very weak nitric acid gives a brown tint to it, exactly like that of an alkali: strong solution of muriatic acid does not affect it much, but sulphuric acid does.

* 6th Edition, Vol. I. page 482.

At Vol. VI. p. 152, and XI. p. 403, I have shewn the effect of boracic acid, in reddening turmeric paper. Mr. South, I believe, first shewed, that the sub-acetate of lead reddened turmeric, and this has been considered sufficient evidence by many, that the oxide of lead merited, in some degree, the name of an alkali. I find on trial, however, so many substances possessing this property, that it must either be limited more exactly than has yet been done, or else given up as a distinguishing property.

All the soluble salts of iron that I have tried, except the acetate, brown turmeric paper. Weak solutions of the green and red muriate seem very alkaline indeed, and even common green vitriol strongly so. They do not, however, produce the same effect on rhubarb paper, but the per salts give it an olive-green tint, whilst the proto salts produce no change at first, but gradually give green tints, apparently from becoming per salts.

If a strong solution of muriate of zinc be boiled on zinc, it gradually oxidizes the metal and dissolves it; and a concentrated solution is obtained which, when diluted, deposits either an oxide or a sub-muriate of zinc. This strong solution is apparently alkaline to tumeric paper. If it be diluted with about its bulk of water, and filtered, it will still redden tumeric paper, though it proves slightly acid by litmus paper. If farther diluted, more precipitate will fall, and the solution will appear alkaline or not, according as the dilution has been small or great. This substance has the same effect on rhubarb paper, and the tint is very like that of a true alkali.

The acid nitrate of bismuth appears alkaline to tumeric; if diluted till a little oxide becomes deposited, it is more so:—the common solution of chloride of antimony in muriatic acid added to water till a precipitate falls, appears alkaline:—per muriate of tin produces a strong change; proto-muriate of tin a very decided redding;, sulphate of tin, slight only. When the acid nitrate of bismuth, and the three salts of tin mentioned are applied to rhubarb paper, they produce but little effect at first, but if dried by the fire become quite brown.

A strong solution of chloride of manganese seems feebly but evidently alkaline to tumeric paper.

It may be supposed, that with many of these substances, the effect is produced principally by the acid present, inasmuch as they were all more or less acid to litmus, and it has been shewn that these bodies, especially nitric acid, have this power. But the very slight excess in some of them, as in the salts of iron, zinc, manganese, *&c.*; and the greater effect produced by farther dilution, as with bismuth and antimony, shew that the whole substance must act: for if it be considered due to the acid alone, it is not, in the first case, of sufficient quantity, and in the second, it would be diminished by dilution.

It is probable that, if the tints produced be compared exactly with those occasioned by alkalies, slight differences might be perceived among some of them; or that, if the properties of the altered colouring matter were examined, they would be found different with the different substances, as M. Desfosses * has shewn with regard to boracic acid and other acids; but my object has not been to trace these changes as far as possible, but merely to shew their general appearance; to guard against any deceptive conclusion with respect to solutions tested by tumeric; and to call attention to the distinguishing characters of acids and alkalies.

Art. X. *Proceedings of the Royal Society.*

The following papers have been read at the table of the Royal Society since our last report.

March 28. Contributions towards the anatomy of whales, by Mr. William Scoresby, communicated by Sir Everard Home, bart., V. P. R. S.

April 18. On the changes that have taken place in the declination of some of the principal fixed stars, by John Pond, esq., Astronomer Royal, F. R. S.

Extract of a letter from Captain Sabine to the President.

Some observations on the buffy-coat of the blood, by John Davy, M. D., F. R. S.

* Vol. xi. 403.

April 25. Some observations made preparatory to the eclipse of 21st August, 1821, and of the eclipse itself, by Mr. William Dawes.

On the mechanism of the spine, by Henry Earle, esq., F.R.S.

May 2. On the nerves which associate the muscles of the chest in the actions of breathing, speaking, and expression, by Charles Bell, esq.

A short account of some appearances in the moon, 24th April, 1822, by Henry Lawson, esq.

9. Experiments and observations on the pitch stone of Newry, and its products, and on the artificial formation of pumice, by the Right Hon. George Knox., F. R. S.

16. Observations on the changes the egg undergoes during incubation in the common fowl, by Sir Everard Home, bart., V. P. R. S.

23. On the mathematical laws of electro-magnetism, by Peter Barlow, esq.

Observations on the heights of places in the trigonometrical survey, by B. Bevan, esq.

June 6. On the binomial theorem, by John Walsh, esq.

Some observations on corrosive sublimate, by John Davy, M.D., F.R.S.

13. On the state of water and aëriform matter in the cavities of certain crystals, by Sir Humphrey Davy, Bart. P. R. S.

20. Some experiments on the changes which take place in the fixed principles of the egg, by Wm. Prout, M. D., F. R. S.

27. On the ultimate analysis of animal and vegetable substances, by Andrew Ure, M. D., F. R. S.

Art. XI.—ANALYSIS OF SCIENTIFIC BOOKS.

i. *De l'emploi du Chalumeau, dans les Analyses Chimiques, et les Déterminations Minéralogiques. Par* M. Berzelius. *Traduit du Suedois, par* F. Fresnel. *A Paris*, 1821.

The Use of the Blow-pipe in Chemical Analysis, and in the Examination of Minerals. By J. J. Berzelius. *Translated from the French of* M. Fresnel, by J. G. Children, F. R. S., &c, *London*, 1822.

In the first volume of this Journal, we noticed a former work by M. Berzelius, entitled "An Attempt to establish a Pure Scientific System of Mineralogy," &c., and gave our opinion very decidedly, of the complete failure of that "attempt." It is highly gratifying, that we are enabled to make a very different report of the work now before us, and to speak almost as much in its praise, as we did in censure of its predecessor. Before we proceed to the "blow-pipe," however, we must have a few more words, and they shall be the last, with the "attempt."

Our review was of the English translation of that work, by John Black, London, 1814. A copy of the original, Dr. Thomson tells us, in the advertisement, was sent him by Professor Berzelius; on looking over which, "it appeared highly deserving the attention of mineralogists, and likely, if properly followed up, to occasion a most important improvement in the method of analyzing minerals, and in the scientific arrangement of them." He therefore engaged his friend, Mr. Black, to translate the work, who stipulated that the Doctor should compare his manuscript with the original, and "take care that it every where conveyed its sense." "This task," says the Doctor, "*I performed with all requisite attention; and can, I think, answer with some confidence for the fidelity of the translation.*" How carefully he revised his friend's manuscript, and how well founded his confidence in its fidelity to the original was, we cannot decide directly from our own knowledge, as we have not the Swedish copy before us. Berzelius, however, has complained bitterly of its infidelity, in the Introduction to a fresh translation which he caused to be published in French, under his own immediate inspection, in 1816 *, in consequence of the errors contained in the English copy. Let him speak for himself. "La traduction Anglaise du même ouvrage, fut faite malheureusement par quelqu'un *qui n'a sans doute aucune notion de minéralogie, car elle est pleine d'erreurs;* et dans beaucoup

* We had not heard of that translation, when our first volume was published.

d'endroits, *le sens de l'original est tout-à-fait défiguré.* Elle parut cependant précédée d'une préface du Docteur Thomson, *qui assurait l'avoir confrontée à l'original, et qu'elle était parfaitement fidèle.* Sur ce temoignage, le redacteur du Journal de Physique en fit faire une version en Français qui renferme naturellement toutes les fautes de la traduction Anglaise. C'est donc par le desir de voir cet ouvrage jugé tel qu'il est, que j'ai publié une traduction d'après l'original Suédois, et sans autres changements que la correction de quelques nombres, que j'ai taché de rendre plus exacts; la substitution du résultat de nouvelles analyses à d'autres moins parfaites, et le remplacement de quelques exemples par d'autres mieux choisis." Under these circumstances, we have felt it our duty to compare the English translation of 1814, with the French of 1816, for which we have the author's assurance in the words just quoted, that it is *really* faithful to the original Swedish, an assertion in which Dr. Thomson necessarily acquiesces, as he has not any where, that we know of, called it in question. We had scarcely begun this hard task, before it was evident that the Black and Co. translation is any thing but literal, and in some instances the sense, even of the passages, is perverted. Dr. Thomson did well to speak with caution of its fidelity. We forbear to quote the parallel passages alluded to, because we do not think it worth while to employ our reader's time or our own so unprofitably; but, if any one doubt our accuracy, he may soon cure his scepticism by referring to the two books. He need not look far for ample proof of the truth of our assertion. The excuse for all this we suppose will be, that they translated from the original Swedish, and we have been comparing their translation with the French. The apology, however, is inadmissible, for Berzelius has declared the French translation to be faithful to its prototype, and we cannot but believe that he is quite as conversant with the Swedish and French languages as Messrs. Black and Thomson are with the Swedish and English.

But the point that chiefly concerns us is, has the English translation perverted the general views of the original work, and especially those passages, which have been commented on by us? For it is, in fact, the original which has been on its deliverance at the bar of critical justice; though the translation comes in of course as accessary after the fact, and must also plead, (guilty, we fear,) to a separate indictment for its own peculiar sins of inaccuracy and error.

The note respecting the quantity of oxygen in the earths, and the method of ascertaining it in silica, is very much shortened in the French translation, the passages quoted by us being wholly omitted. We cannot decide, therefore, who has best right to the nonsense it contains, the author or his translators. It is not necessary, however, as we have said before, to trouble

our readers with particular passages, since, after having carefully compared the two translations, we do not find any sufficient reason to induce us to revoke our opinion of this *nouveau système.*

We have been guilty ourselves of an omission, in the review alluded to, in having left unnoticed the *signs* which Berzelius has invented, to express the composition of chemical compounds and minerals; we shall therefore devote a few words to them in this place. "When we attempt," says our author, "to express chemical proportions, we perceive the necessity of having chemical signs." (*Essai sur la Theorie des Proportions Chimiques*, p. 109,) and accordingly he adopts two kinds, one chemical, the other mineralogical. With the former, 1st. a simple substance not metallic (which he calls a metalloid), is denoted by the initial letter of its Latin name, although it may be also common to the name of some metal; 2d. a metal having the same initial as another metal or a metalloid, is denoted by the two first letters of its name, or should these be common to some other name, then the first different consonant is added to the initial letter instead. Thus O. = oxigenium, S. = sulphur, Si. = silicium, St. = stibium, Sn. = stannum, *&c.* A chemical sign only indicates a single atom; to denote a greater number, a figure is placed to the left of it, like an algebraical coefficient: thus Cu + O, signifies protoxide of copper, and Cu + 2 O peroxide of copper. But since oxygen enters into most combinations, and often in the proportion of several atoms, instead of employing its proper sign O, with or without its coefficient, the number of its atoms is indicated by points placed over the base, each point denoting an atom of oxygen. Thus $\dot{Cu}$ = protoxide of copper, $\ddot{Cu}$ = peroxide of copper, $\ddot{S}u$ = sulphurous acid, and $\dddot{S}u$ = sulphuric acid, *&c.* Proto-sulphate of copper is expressed by combining these two signs, as $\dot{Cu}\,\dddot{S}$, and the persulphate by $\ddot{Cu}\,\dddot{S}^{2}$ the exponential figure, ^{2} denoting that it contains two atoms of acid. The composition of a salt with more than one base is expressed by connecting the signs by the usual algebraic mark of addition. Sulphate of potassa and alumina, for instance, is denoted by $\ddot{K}\,\dddot{S}^{3} + 2\,\ddot{\dddot{Al}}\,\dddot{S}^{3}$; (K being assumed to represent *kalium*, i. e., potassium,) and crystallized alum, in which these ingredients are combined with several atoms of water (expressed by $H^{2}O$.) is represented by $(\ddot{K}\,\dddot{S}^{3} + 2\,\dddot{Al}\,\dddot{S}^{3}) + (48\,H^{2}O.)$ (See Essai, p. 113) The mineralogical signs are formed on a similar model, but are more simple. They consist of the initials of the elementary bodies in the italic character, the dots denoting the atoms of oxygen are omitted, and water is represented merely by Aq. Black Siberian Mica for instance is denoted

by $K S^3 + 2 M S + 3 A S + 4 F S$ meaning that it is composed of one atom of trisilicate of potassa, two atoms of silicate of manganese, three atoms of silicate of alumina, and four atoms of silicate of iron! Now we beg to dissent *in toto* from the assertion that these or any other signs are at all necessary, and we contend that they are calculated rather to mislead and mystify, than facilitate our progress, or elucidate our results. If we gain any thing in brevity, (and very little do we even in that respect) it is at the expense of great risk of confusion, both from that misinterpretation which all symbols are liable to, as well as from the inaccuracies they are likely to introduce, by errors in transcribing, or of the press. Even in point of brevity, the advantage is merely in the act of committing them to paper, for when they are to be deciphered, we must, mentally at least, read them at full length, if we would perfectly comprehend their import. It is not with these symbols as with the figures in arithmetic, for they are connected with the various operations of addition, subtraction, and multiplication; which could hardly be performed, were the quantities expressed by words. A peculiar method of abbreviated notation therefore in that case is absolutely necessary, but the chemical and mineralogical symbols, are mere expressions of simple aggregates, and when once noted down are capable of no further operation. Hence common language is amply sufficient for every purpose these symbols can be applied to. There is, besides, a certain air of mathematical parade in these symbols which is not pleasing; the connecting sign +, with their coefficients and exponents, gives them so strong a resemblance to algebraical formulæ, that on opening at some of the pages of the *Nouveau Systeme*, we might almost fancy we had taken up a volume of Bonnycastle's Algebra.

Before we quit the subject of mystification, we shall say a few words on a propensity which has sprung up since the establishment of the atomic theory, to entangle chemical calculations in unnecessary algebraical formulæ. In the sixth volume of the *Annals of Philosophy*, p. 321, there is a highly interesting and valuable paper on the relation between the specific gravities of bodies in their gaseous state, and the weights of their atoms. The name of the author is not annexed, but it is now well known to be by Dr. Prout. We select this paper as a case in point, (though similar instances are but too numerous) because its excellence demands that it should be universally known and understood, which, as the calculations whence the results are derived are algebraical, it can hardly be by all who cultivate the science of chemistry. These calculations are not, indeed, absolutely necessary to render the general object of the essay intelligible, and are therefore subjoined in the form of notes; but, however confidently the reader may rely on the accuracy of an author's

results, it is always satisfactory as well as instructive to trace the steps by which he arrives at them; and hence it is desirable that they should be detailed in the plainest possible form, and when the subject will admit of it, and it does not involve tedious and periphrastic operations, by the method of common arithmetic. Now every one of the calculations in the notes to this paper, might just as well have been worked by the common rule of three, as by algebra. For instance, let us take the first calculation. Atmospheric air is constituted by bulk of four volumes of azote, and one volume of oxygen, " and if we consider the atom of oxygen as 10, and the atom of azote as 17.5, it will be found to consist by weight of one atom of oxygen and two atoms of azote, or per cent.

Oxygen	22.22.
Azote	77.77.

From these data the specific gravities of oxygen and azote, (atmospheric air being 1.000) will be found to be,

Oxygen	1.1111
Azote	0.9722" (An. Ph. vi. 322.)

To which the following formula is subjoined.

Let x = sp. gr. of oxygen. $22.22 = a$.
y = sp. gr. of azote. $77.77 = b$.

Then $\frac{x + 4y}{5} = 1$

And $x : 4y :: a : b$.

Hence $5 - 4y = \frac{4ay}{b}$

And $y = \frac{5b}{4a + 4b} = 0.9722$, and $x = 5 - 4y = 1.1111$.

Now since there are five equal volumes of gases in the mixture, which together compose the atmospheric air whose specific gravity is unity, the specific gravity of each of the component gases must be in the ratio of the weight of one volume of either of them respectively, to that of $\frac{1}{5}$th of the volume of the compound produced; for there is no change of volume on their being mixed; that is, calling the volume of air produced 100.

20 : 22.22, &c. :: 1.00 : 1.1111
and 20 : 19.44, &c. $\frac{(77.77)}{4}$ :: 1.00 : 0.9722.

And so for the other calculations of the specific gravity of hydrogen, chlorine, &c.

It will hardly be supposed that we mean to question the importance of algebra in a general sense; no one can be more conscious than ourselves, how indispensably necessary it is to have not merely a slight, but a full and perfect knowledge of that science in a great variety of complicated and abstruse investigations; it is only in cases like the present, where no *dignus vindice nodus* exists, that we protest against its introduction, for even such a simple formula as the preceding tends to perplex a numerous class of readers, and, as to them at least, to obscure rather than illustrate. Plain facts should be stated in plain terms; to introduce extraordinary means for accomplishing ordinary ends, is like Hogarth's philosopher using complicated machinery to draw a cork. We would say therefore to the mathematical chemist, enjoy the advantage which your algebra gives you in your closet, avail yourself of all its powers to abbreviate your calculations and elucidate your theoretical speculations, but when you detail them to the general mind, let them be clothed in the plainest garb your genius can invest them with; and in all cases, unless it be absolutely necessary, instead of saying let x be this or that, we say let x alone.

We perfectly agree with Dr. Thomson in the well-merited eulogy he has pronounced on Dr. Prout for the essay alluded to, (*Annals of Philosophy*, xvi. 167); but when he asserts that "mere experimenters may relinquish the field, for there is not a great deal more which they can do," we consider him as endeavouring with the whole weight of his authority, whatever it may be worth, fatally and effectually to oppose the progress of chemical science. If by the mere experimenter he mean a foolish fellow, who swelters at the furnace, and half suffocates himself with the fumes from his sand bath without any determinate object in his labours, or with views as chimerical as the philosopher of Laputa who tried to make deal boards out of saw-dust, well and good! but if he mean, as he obviously does, that a knowledge of algebra and the mathematics is *essentially necessary* to the *chemist*, we deny the assertion. An ingenious and skilful operator thoroughly conversant in chemical philosophy, but with merely a competent knowledge of common arithmetic, is as likely to devise and execute important experiments as the best algebraist that ever combined the knowledge of the two sciences. The whole of Dr. Black's theory of latent heat is developed in his lectures edited by Dr. Robison, without the introduction of a single algebraical formula in the text; whence it is clear that Black did not think algebra necessary to explain his views, even on so difficult a subject. Would Dr. Thomson, who sets himself up as the autocrat of chemistry, have presumed to have ordered *him* to relinquish the field? But it is time to turn to the volume that lies open before us, and we do so with pleasure.

"The subject of the work now offered to the public," says

the author, very truly, in his introduction, "is highly interesting to the practical chemist, the miner and the mineralogist. It is a system of chemical experiments, made in the dry way, as it is called, and almost always on a miscroscopic scale, but which presents us in an instant with a decisive result." It is not less true, that the way in which he has treated it, is as satisfactory as the subject is important. Mr. Children (whose translation we shall more particularly notice hereafter, adopting it generally in the mean time, in the passages we may have occasion to quote) observes in his preface, "The name of Berzelius, as a skilful and patient experimenter, stands almost unrivalled; and the present essay amply vindicates his claim to the high reputation he has acquired. It is an invaluable collection of important and new facts, and admirably supplies the want which has long been felt and acknowledged of a scientific practical treatise on the blow-pipe." (Trans. preface, v.) The observation is just in every respect.

The subject is treated in the following order. History of the blow-pipe.—Description of the blow-pipe.—The combustible—the blast and flame—the support—additional instruments—the re-agents and their use—the habits of the pure alkalies, earths, metallic oxides, sulphurets, alloys and acids, before the blow-pipe, and the pyrognostic characters of minerals and urinary calculi.

Lamps are recommended to be used with the blow-pipe, in preference to candles, as "the radiant heat from the substance under examination melts the tallow or wax, and occasions them to burn away too fast; and besides common candles do not always furnish sufficient heat." "The best fuel for the lamp is olive oil."

Under the head "*blast* and *flame*," are given very ample and accurate directions how to keep up a steady blast, and produce a strong heat, and the following instructions for oxidation and reduction are very important.

"*Oxidation* ensues when we heat the subject under trial before the extreme point of the flame, where all the combustible particles are soon saturated with oxygen; the farther we recede from the flame, the better the oxidation is effected (provided we can keep up sufficient heat;) too great a heat often produces a contrary effect, especially when the assay is supported by charcoal. Oxidation goes on most actively at an incipient red heat. The opening in the beak of the blow-pipe must be larger for this kind of operation than in other cases.

"For *reduction*, a fine beak must be employed, and it must not be inserted too far into the flame of the lamp; by this means we obtain a more brilliant flame, the result of an imperfect combustion, whose particles, as yet unconsumed, carry off the oxygen from the subject of experiment, which may be considered as

being heated in a species of inflammable gas. If in this operation the assay become covered with soot, it is a proof that the flame is too smoky, which considerably diminishes the effect of the blast. Formerly, the blue flame was considered as the proper one for the reduction of oxides, but this idea is erroneous; it is in reality the brilliant part of the flame which produces deoxidation; it must be directed on the assay so as to surround it equally on all sides, and defend it from the contact of the air."

"A very advantageous mode of practice, in order to acquire the art of making a good reducing flame, is to fuse a small grain of tin, and raise it to a reddish white heat on a piece of charcoal, so that its surface may always retain its metallic brilliancy. Tin has so great a tendency to oxidation, that the moment the flame begins to become an oxidating one, it is converted into an oxide of tin, which covers the metal with an infusible crust. We must begin by operating on a very small grain, and gradually proceed to larger and larger. The greater the quantity of tin that he can thus keep in a metallic state, at a high temperature, the more expert is the operator in his art."

The supports are charcoal, platinum foil, platinum wire, plates of mica, glass tubes, open at both ends, and glass matrasses.

The best of these, except in particular cases, is the platinum wire; and we can confirm, by our own experience, its superior convenience. "The diameter of the platina wire is arbitrary; the finest is the best, provided it be thick enough not to bend with the blast; if too thick, it absorbs too much heat." We find a wire of 1—100th of an inch in diameter, answer the purpose excellently. The mode of using it is thus described: It is to be "bent at one end into a hook, which serves as the support in the following manner. Having moistened the hook with the tongue, it is to be dipped into the flux, a portion of which will adhere to it; this is to be fused by the lamp into a globule, which congeals and adheres to the curvature; the assay must then be moistened, to make it adhere to the flux, which is now solid, and the whole heated together. We thus obtain an insulated mass, which may be conveniently examined without danger of our being deceived by the appearances which sometimes ensue on charcoal from the play of colour, when the assay globule is detached on a black ground. This method is much superior to either of the preceding, and so completely answers the purpose, that, in most cases, the platina wire is preferable to charcoal, especially when the reduction of a metallic oxide is to be avoided. Generally speaking, all oxidations should be performed with the platina wire, as well as those reductions in which change of colour is the only object in view."

"*Plates of mica* may be used in roasting ores, when the de-

oxidating effect of charcoal on the parts of the assay in contact with it, might be injurious.

"Glass Tubes.—When it is necessary to roast a substance to ascertain what it is combined with, I use a glass tube, two inches long at least, and about one-eighth of an inch in diameter, and open at both ends; I introduce the assay into it at a little distance from one of the ends, and, according to the temperature required, I heat the tube either with the spirit lamp or with the common lamp by means of the blow-pipe, inclining it more or less (the part containing the assay being held downwards) as it is necessary to pass a more or less rapid current of air over the assay. The volatile substances, not permanently gaseous, sublime, and condense, in the upper part of the tube, where their nature may be ascertained." When charcoal is used as the support, the author directs the assay to be laid with its flux on "one of the ends perpendicular to the layers of the wood; if placed on the section parallel to the layers, it would spread over the surface. Although the space between the woody layers may consume faster than the layers themselves, there is this advantage attending it, that the assay rests in that case merely on their summits, and thus often has only two or three points of contact with the charcoal."

The re-agents are carbonate of soda, borax, salt of phosphorus, (double phosphate of soda and ammonia), saltpetre, vitrified boracic acid, gypsum and fluor spar, nitrate of cobalt, tin, iron, lead, bone-ashes, silica, and oxide of copper. The three first are the principal, the others are only wanted occasionally. It is absolutely necessary that the re-agents should be quite pure, for obtaining them in which state, and the method of applying them, ample directions are given. Those respecting the use of soda are very minute and valuable. This re-agent is of great service in the fusion of bodies, and the reduction of metallic oxides. Gahn's method of discovering very minute portions of metal, reduced on charcoal by means of soda, deserves particular attention.

Borax is employed to effect the solution or fusion of a great number of substances. Its use "is founded on the tendency of its component parts to form compounds that are all fusible, though in different degrees. On one hand, it dissolves bases, and forms with them a fusible double salt with excess of base; on the other, it dissolves acids, amongst which I place silica, and even to a certain extent alumina, and forms with them acid and fusible double salts. As all these salts commonly preserve their transparency on cooling, we can hence judge the more certainly of the colour which the compound acquires from the substance dissolved."

Salt of phosphorus "acts as a re-agent, principally by

means of its free phosphoric acid; and the advantage of employing this salt, rather than the acid itself is, that the latter is very deliquescent, much more costly, and is readily absorbed by the charcoal. The salt of phosphorus, therefore, shews the action of acids on the assays; its excess of acid seizes on all their bases, and forms with them more or less fusible double salts, whose transparency and colour may then be examined. Consequently, this flux is more particularly applicable to the examination of metallic oxides, whose characteristic colours it developes much better than borax.

"The same flux exerts a repellant action on acids. Those which are volatile sublime, whilst the fixed acids remain in the mass, and divide the base with the phosphoric, or yield it entirely to it; in the latter event, they remain in suspension in the glass, without dissolving in it. In this respect the salt of phosphorous is a good re-agent for the silicates; it sets the silica free, which then appears liquefied in the salt as a gelatinous mass."

Saltpetre is useful in detecting small portions of manganese. Vitrified boracic acid, in conjunction with iron, for ascertaining the presence of phosphoric acid; gypsum and fluor spar, are used mutually to detect each other, and have no other use. Nitrate of cobalt in solution in water, is employed to ascertain the presence of alumina and magnesia; with the former it gives a blue colour, with the latter a pale rose colour. The directions given for applying this test must be carefully attended to, to avoid equivocal results. Tin, in the state of foil, is used to promote the reduction of metallic oxides; iron, to precipitate certain metals, as copper, lead, nickel, and antimony, and to separate them from sulphur, or fixed acids. Lead is employed in cupellation with bone-ashes.

"The bone ashes must be reduced to a very fine powder, a small quantity of which is to be taken on the point of a knife moistened with the tongue, and kneaded in the left hand, with a very little soda, into a thick paste. A hole is then made in a piece of charcoal, and filled with the paste, and its surface smoothed by pressure with the agate pestle. It is then to be gently heated by the blow-pipe, till it is perfectly dry, (the soda only assists the cohesion, and may be omitted). The assay, previously fused with lead, is placed in the middle of the little cupel, and the whole heated by the exterior flame. When the operation is finished, the precious metals are left on the surface of the cupel. This experiment is so delicate, that grains of silver visible to the naked eye, and indeed such as may be collected by the forceps and extended under the hammer, may in this way be extracted from the lead met with in commerce."

Silica, fused into a glass with soda, detects the presence of sulphur or sulphuric acid; and oxide of copper is used to de-

tect the presence of muriatic acid. The application of the last test is very ingenious.

"We fuse oxide of copper with salt of phosphorus into a dark-green globule, we then add the assay, and heat the whole before the blow-pipe. If it contain muriatic acid, the globule is surrounded by a fine blue flame, inclining to purple, which continues as long as any muriatic acid remains in the assay. Not one of the other mineral acids produces a similar phenomenon, and such of them as form cupreous salts, which do of themselves colour the blow-pipe flame, lose that property when combined with salt of phosphorus. For instance, the earthy mineral, in which the blue carbonate of copper (from Chessy, in France) occurs, communicates an intense green colour to the flame when heated before the blow-pipe; but when treated with salt of phosphorus, previously saturated with oxide of copper, not the slightest colour is any longer perceptible in the flame."

Under the head of *General Rules for Experiments with the Blow-pipe*, much valuable information as to the size of the assay, and the order to be observed in experimenting, is given, which our limits will not allow us to detail. We recommend a careful perusal of them to our readers.

The next division treats of the phenomena presented by different mineral substances before the blow-pipe. The experiments were made, as the author assures us in his introduction, on pure substances, and present a number of instructive and valuable facts. The effects produced by the fluxes on oxide of titanium, both pure and mixed with iron, and the means of distinguishing it from the glasses of ferruginous tungstic acid, ferruginous antimonious acid, and oxide of nickel, which assume the same shade as ferruginous oxide of titanium in the reducing flame, are very minute and important.

Oxide of cadmium, "on charcoal is dissipated in a few seconds, and the charcoal is covered with a red or orange-yellow powder. This phenomenon is so marked with oxide of cadmium, that minerals, which, like carbonate of zinc, contain one or two per cent. of carbonate of cadmium, when exposed for a single instant to the reducing flame, deposit, at a little distance from the assay, a yellow or orange-coloured ring of the oxide, most distinct when the charcoal is cold. This ring forms long before the oxide of zinc begins to be reduced, and if flocculi of that metal appear at the same time, it is a proof that the blast has been pushed too far; but if we can discover no yellow trace before the fumes of zinc begin to condense on the charcoal, we may conclude that the assay contains no cadmium."

Mr. Children has added the following in a note, on this subject, which he had from the late Dr. E. D. Clarke. "If platina foil be used for the support, instead of charcoal, the yellow or

orange-coloured ring is not only conspicuous upon the platina, but by exposing it to the point of the blue flame, the metal is revived, and afterwards deposited upon the platina, during its combustion, in the form of a protoxide, exactly like polished bronze or copper *."

The method of distinguishing bismuth from antimony and tellurium is very satisfactory, as well as the following for detecting very minute portions of tin and copper. "With soda, on the platina wire, oxide of tin combines with effervescence, forming an infusible turgid mass, insoluble in an additional quantity of soda. On charcoal it is readily reduced into a globule of metallic tin. Some native oxides of tin, especially such as contain columbium, are not easily reduced with soda, so that, on a first experiment, one might doubt its presence; but if we add a small quantity of borax, the reduction is immediately accomplished.

"Tin often occurs in nature as an accidental, and relatively, very small constituent part of the ores of columbium, titanium, and uranium, and perhaps of some others, where its presence would be little suspected in experiments made in the moist way; but when treated with soda in the reducing flame, particularly after separating the iron, metallic tin is always detected, even though it enter in no larger proportion than 1-200th part of the weight of the ore. If the proportion of iron be inconsiderable, its reduction may be prevented to a certain point, by adding borax to the soda."

Oxide of copper, "with salt of phosphorus, fuses and exhibits the same shades as with borax. If the proportion of copper be inconsiderable, the glass sometimes becomes transparent and ruby red, after exposure to the reducing flame, nearly at the moment of congelation. Commonly the glass becomes red and opaque like enamel. If the quantity of copper be so small that the character of the protoxide cannot be developed by the reducing flame, we add a little tin to the assay, (whether the flux be salt of phosphorus or borax,) and immediately continue the blast. The before colourless glass now becomes red and opaque on cooling. If the blast be kept up too long, the copper precipitates in the metallic state, particularly with salt of phosphorus, and the colour is destroyed.

"With soda on the platina wire, oxide of copper fuses into a fine green glass, which loses its colour and transparency on cooling. On charcoal the mass is absorbed and the oxide reduced. There is probably no other possible method of discovering such minute proportions of copper as may be detected by the blow-pipe, in all cases where it is not combined with other reducible metals, liable to disguise its properties. In the

* If we not mistake, the invention of this process should be ascribed to Dr. Wollaston.

latter case we must employ borax and tin. If copper and iron be found together, the same operation reduces each separately into distinct particles, which may be known by their respective colours, and separated by the magnet."

The methods of examining the sulphurets, arseniurets, and alloys, and the characteristic effects of acids, as component parts of salts, are given with equal perspicuity and detail, and the mode discovered by the author of detecting phosphoric acid, by means of boracic acid and iron, is ingenious and useful.

The last division is devoted to the pyrognostic characters of minerals and urinary calculi, and constitutes the largest portion of the work. In order to shew our readers the way in which this important part of the subject is treated, we subjoin an instance, taken at random, with which we shall conclude our extracts.

"Chondrodite, from Pargas, Åker and America. The Brucite of the Americans*. Alone, in the matrass, blackens by heat, but gives no appreciable quantity of water; the black colour disappears by roasting in the open air.

"On charcoal it is infusible. The most ferruginous chondrodite becomes opaque and brown on the points that are most strongly heated. The variety containing less iron, that from Åker, for instance, becomes milky white, by the effect of heat.

"With borax fuses slowly, but completely, into a transparent glass, slightly tinged by iron. If the glass be saturated with the chondrodite, it loses its transparency by flaming; it does not, however, become milky white, but semi-translucid, and crystalline.

"With salt of phosphorus decomposes pretty easily, and leaves a semi-transparent siliceous residuum; the glass is clear and colourless, but becomes opaline on cooling. A small quantity of soda transforms it into a difficultly fusible grey scoria; with a larger quantity it intumesces and becomes infusible.

"With solution of cobalt, in a strong heat, it gives a pale red colour, not pleasing to the eye. Chondrodite from Pargas gives a brown grey, the action of the iron preventing that of the oxide of cobalt on the magnesia."

The portion of this work devoted to the examination of urinary calculi, though brief, will be found of great utility and importance, especially to medical men, to whose attention we earnestly recommend it, as "simple and infallible, and requiring no more chemical knowledge than every physician ought to possess."

* "*Silicate of Magnesia.*—Almost all the magnesian silicates contain a combustible substance, which becomes charred when heated in close vessels. If the mineral be then heated in the open air, the charcoal burns away, and the black colour disappears. Steatite is a remarkable instance." B.

To this general analysis, and the copious extracts which we have given of the work before us, it is unnecessary to add much more; the latter prove our opinion of its merits, for we should not have thought it necessary to lay so much of its contents before our readers, were we not desirous by the specimens we have furnished to induce them to form a more intimate acquaintance with the work itself, and we have no hesitation in assuring them that they will be amply rewarded for their trouble. The part we like least is the mineralogical arrangement adopted by the author, the continual recurrence of his abominable formulæ, and his whimsical speculations on isomorphous bases, which have led him into some tedious and unprofitable details on the subject of the amphiboles, pyroxenes and garnets. Mr. Children has, very properly in our estimation, wholly omitted the formulæ, translating them into plain English in notes at the bottom of the page; we wish he had exerted the same discretionary judgment with respect to the *isomorphisms*, and left them out likewise. It remains to say a few words respecting the translation.

In the first place, with respect to its fidelity, we have been at the pains of carefully comparing it with the original, and have found it at once clear and correct. The original is closely adhered to, except in a few instances of trifling omissions, and others of judicious curtailments to which we have above adverted, which it is unnecessary to particularize, as the translator has fully explained his reasons for them in his preface. We wish that it were more common, for gentlemen of Mr. Children's rank and attainments in science, to favour the world with similar productions, for there are few means better calculated for the promotion of natural knowledge, than translations of the works of learned foreigners, accompanied with notes and illustrations by those who are equally acquainted with the subjects of which they treat, and with their authors; and who will condescend to add their stock of information to that which has been collected by others.

We scarcely regretted, though (considering the quarter whence it originated) we were somewhat surprised, at the mistake of the *rule of two*, or the having overlooked the instances where the author, *for theoretical reasons*, has doubled the atomic numbers. It is an admirable illustration of the danger which we have so often pointed out, of the system of halving and doubling *ad libitum*. The error, as Mr. Children observes, (for it was discovered in time to be corrected in a note, though not in the text,) is wholly and absolutely unimportant to the main object of the work; and we cordially agree with him, that "though it certainly is no excuse for his fault, it is one proof amongst a thousand of the danger of involving plain matter of fact in unnecessary hypothetical dogmas."

ii. Dr. Thomas Thomson—*and his* "Answer."

It is the practice of every vice, to assume the garb and name of its neighbour virtue. Thus avarice becomes frugality; chicanery, knowledge of the world; violence of temper, warmth of feeling; rudeness of manners, plain dealing; envy and detraction, the love of truth and justice. We have been presented with an amusing specimen of this masquerade, in Dr. Thomson's pretended "Answer" *, to our Review of his System, published in this Journal fifteen months ago; and were we actuated by personal hostility, we should have rejoiced at its obliquity and intemperance. We apprehend that the misstatements and abuse with which he has sullied the pages of Mr. Phillips's Journal, are the signs of a desperate cause; they are, at all events, ill calculated to erase the impression which our Review has made upon the public mind respecting the merits and accuracy of the "System."

In presenting our readers with a few additional remarks in reply to Dr. Thomson's *Answer*, we shall not degrade either ourselves, or our Journal, by imitating the coarseness of his invective, nor shall we delay until we have availed ourselves of the Regius Professor's obliging invitation to attend his lectures at Glasgow—having already had the gratification of listening to a portion of one of his discourses at the Surry Institution, near Blackfriar's Bridge, London. We shall, therefore, at once proceed to the business before us; and we pledge ourselves to defend every statement, and corroborate every assertion of our former review.

In the first place we have accused Dr. Thomson of speaking too favourably of himself, but we are sorry to observe from his "Answer," that he has not profited by our advice; and, that he is forgetful of the old proverb "*propria laus sordet*," of the truth of which he furnishes a most diverting example.

Speaking, for instance, of his *System* he says, it is a work "which has in some measure stamped the character of every systematic treatise, *both in Britain and America, and even on the continent of Europe*; and which has been sanctioned by the almost unqualified approbation of the most eminent chemists in Britain, France and Germany." Of his own individual accomplishments he says in the same tone, "As I have uniformly prided myself in the honesty, sincerity, and independence of my character; as I have been at considerable pains to give credit to whom credit was due; as I have uniformly both in my System, and in the Annals of Philosophy, while I continued its editor, given the merit of every chemical fact to

* Annals of Phil. April 1822.

the original discoverer of it, as far as my knowledge of the subject enabled me to go; as I am not conscious of any wilful misrepresentation, or twisting of facts, to serve any particular purpose; I should consider myself guilty of a kind of *felo-de-se*, if I were not to step forward in the present case in my own vindication *."

Let us next hear his own opinion and eulogium of his own *style*. "I may be very often accused of great carelessness of style; but, *never*, unless I deceive myself egregiously, either of want of energy or diffuseness. *Indeed the characteristic properties of my style are just the opposite of diffuseness. I am remarkably concise, though I hope always clear, and generally energetic* †."

One would fancy on reading the above, that it was a fragment of some Academician's eulogy on the style of Tacitus or Montesquieu. But the passage itself is a good example of Doctor Thomson's incessant tautology. And by what splendid performance has he earned his privilege of being often guilty of great carelessness of style; and how is this, his acknowledged and habitual vice in writing, compatible with conciseness, clearness and energy?

Of the great versatility of Dr. Thomson's powers of expression, our readers may judge by the two following samples. If the first be reckoned diffuse, the second will be allowed to be more energetic, though it may be doubted how far he is entitled to credit for the vigour of its diction. "Such, as far as I am acquainted with them, are the changes produced by vegetation. *But plants do not continue to vegetate for ever*; sooner or later they *decay* and *wither* and *rot*, and are *totally decomposed*. This change, indeed, does not happen to all plants at the end of the same time Some live only for a *single* season, or even for a shorter period; others live *two* seasons; others *three*; others a *hundred* or more; and there are some plants which continue to vegetate for a *thousand* years; but *sooner* or *later* they all cease to live; and then these very chemical and mechanical powers, which had promoted vegetation, combine to destroy the remains of the plant. Now what is the cause of this change? *Why do plants die?* This question can only be answered by examining with some care what that is which constitutes the life of plants; for it is evident that if we can discover what that is which constitutes the life of a plant, it cannot be difficult to discover whatever constitutes its death. Now the phenomena of vegetable life are, in general, *vegetation*. As long as a plant continues to vegetate, we say that it lives; when it ceases to vegetate, we conclude that it is dead ‡." This mode of writing, so concise, clear and energetic

* Annals of Phil. April, 1822, p. 242. † Answer, ut supra, p. 245.
‡ System, Edition 6th, iv., p. 361. In quoting this before, we did injustice to our readers, by omitting the earlier portion of the passage.

in the author's mind, we ventured to denominate the *vacuous* *, and gave numerous examples equally remarkable with the above. The Doctor is astonished at the inappropriateness of the epithet; and to prove that he can be concise, clear and energetic, he has lately favoured the world with the following document. "Having been led by very false information, to accuse most unjustly Thomas Allan, Esq., Banker, in Edinburgh, on a subject connected with his mineralogical pursuits, I now *publicly* express my sincere regret for having propagated a *most groundless calumny* against that gentleman, and do declare, that I now find that so far from what was reported to me, and repeated by me, having the slightest foundation, Mr. Allan, on the contrary, was the direct means of tracing and transmitting to the proper owner in London, the minerals which were the subject of the charge." (Signed) *Thomas Thomson.* Vide Constable's Edinburgh Magazine for March, 1822, p. 408, article entitled "Jury Court, Jan. 21, Allan *v.* Thomson—Apology for Defamation." "Mr. Jeffery (Advocate) said, he had only to regret, that this accommodation had not been entered into at an earlier stage, but that it had been delayed till the latest moment (the day of trial,) that such a measure was practicable; he added, that as the delay was to be imputed to Dr. Thomson, it was understood that gentleman should defray the whole expense incurred. This the Counsel for Dr. Thomson acquiesced in, it being stated by the Court that it was a matter of private arrangement †." Mr. Allan, as all our scientific readers know, is a gentleman distinguished by the excellence of his mineralogical collection, the beauty of its arrangement, and the complaisance with which he lays it open to men of science.

That our judgment concerning the general merits of Dr. Thomson's manner of systematizing chemistry, is in unison with that of the leading chemists of the age, we have abundant documents to prove. We shall content ourselves with a few extracts from the writings of Professor Berzelius, a gentleman whom the Doctor would wish to number in his list of friends, and against whose knowledge of the subject, and amiable temper, he can have nothing to object. In the Professor's paper on the composition of the oxides of platinum and gold, inserted in the *Annales de Chimie et de Physique*, for Oct. 1821, we observe the following exposure of Dr. Thomson's mode of perverting chemical results. "Dr. Thomson, however, in the 7th (6th) edition of his System of Chemistry, adopted the peroxide of Mr. Cooper, rejected that of which I had made the analysis, and of which I had described the combinations with the acids and alkalis, and *formed* on the analytical *data* of Mr. Cooper, a tritoxide of platinum in the following manner:—' Mr. Edmund

* See our Review *passim*. † Constable's Mag. *ut supra*.

Davy has found that 100 parts of platinum combine with 11.86 parts of oxygen; and on the other hand, M. Berzelius has determined that 100 parts of this metal combine with 16.494 parts of oxygen. The mean of the results of Edmund Davy and Berzelius, would be 14.177, for the oxygen in the peroxide; now this quantity does not differ much from 13.269, which represents the quantity of oxygen which would be necessary to form a tritoxide.' "*This method of treating the Science of Chemistry,*" adds Mr. Berzelius, "*is peculiar to M. Thomson* *."

Well may the Swedish Philosopher say so. This travesty of experimental results is of a piece with that account of the chlorocyanic acid of M. Gay Lussac, which has excited so much ridicule among the French Chemists. Our Doctor actually describes as a liquid, what M. G. Lussac analyzed in the gaseous form; and he enjoins as the mode of making it, a method of verifying its nature after it is made. No man could prepare it by Dr. Thomson's misdirections.

The Swedish Chemist, in his remarks on the prussiates published in the 15th volume of the *Annales de Chimie et de Physique*, has the following passage: "This is the first example," says Dr. Thomson, "which does not agree with chemical theory; I therefore invite chemists to seek for its explanation, because nothing contributes so much to the advancement of science, as the development of what may appear to contradict opinions already received." To which quotation Berzelius subjoins, "he ought to have added, with the exception of those cases *where the contradiction is the result of an experiment ill-made and inexact, whereby the Science makes a very negative gain* †."

As the public have it in their power to compare at leisure our Review with the Regius Professor's Answer, we had resolved at first to leave the matter entirely in their hands, without further comment, perfectly tranquil as to the final decision.

* "Les expériences analytiques de M. Cooper, ainsi que les conclusions qu'il en a tirées, me parurent ne mériter aucune attention. Cependant M. Thomson, dans la septième édition de son *Système de Chimie*, adopta le protoxide de M. Cooper, rejeta celui dont j'avais fait l'analyse, et dont j'avais décrit les combinaisons avec les acides et les alcalis, et forma sur les données analytiques de M. Cooper, un tritoxide de platine de la manière suivante; M. Edmond Davy, a trouvé que 100 parties de platine se combinent avec 11.86 p. d'oxigène, et d'un autre côté M. Berzelius a determiné que 100 parties de ce métal, se combinent avec 16,494 part. d'oxigène. Le terme moyen des résultats d'Edmund Davy et de Berzelius serait 14.177 pour l'oxigène dans le peroxide; or ce terme moyen ne diffère pas beaucoup de 13.269, qui représente la quantité d'oxigène qui serait nécessaire pour former un tritoxide. Cette méthode de traiter la science est *particulière* à M. Thomson."—*Ann. de Chimie et de Phys.* Oct. 1821, p. 148.

† A quoi, ce me semble il auroit faller ajouter; à l'exception des cas où la contradiction est le resultat d'une experience mal faite et inexacte où la science fait un gain bien negatif. Ann. xv. 147.

But as we conceive some instruction may be derived from an exposure of his recent distortions of scientific truth, we think it our bounden duty as Journalists, to bestow on the Doctor a few parting admonitions, after which we shall leave him in the quiet enjoyment of his self-importance.

We had said in the Review, " We are at a loss to learn why a new edition has come forth; it was not spontaneously called for, and nothing but a decidedly superior work should have been tendered to the public." The Doctor makes answer: " The new Edition, I presume, was printed because the old had been sold. I am not aware that booksellers proceed in any other way *." Now the public should be informed, that during the session immediately preceding the appearance of his sixth edition, his fifth of 1817, was currently rattled off at the Edinburgh book auctions.

To multiply editions in this way, by the aid of the hammer, without the employment of the file, (*limæ labor,*) may suit a book-making factory, but is a great injustice to chemical students, and ought therefore to be exposed and checked. " The assertion," says he, " that the second volume is a repetition of the first, is so palpably untrue, that the Reviewer must have been aware of its inaccuracy when he made it†." Here we have his favourite figure of speech,—perversion of an author's sense. The Reviewer said, in discussing the merits of the second volume, " Before he reaches the combustible acids, one hundred and thirty-three pages are occupied with details, which, to a very great extent, had been already given in the first volume. His unsalifiable oxides are the two oxides of azote, the two of hydrogen, and carbonic oxide, which occupy five sections. These are for the most part repetitions of what he had given us before." Of these book-making repetitions we shall now give some samples.

" If equal volumes of chlorine and hydrogen be put into a glass tube, and exposed to the direct rays of the sun, an explosion takes place. This curious fact was first observed by Gay Lussac and Thenard. When two equal glass vessels, ground so as to fit each other, and filled, the one with dry chlorine, and the other with hydrogen, are placed in contact, and exposed to the light of day, but not to sunshine, the yellow colour gradually disappears, and the mixture becomes colourless. If it be now examined, it will be found converted into pure muriatic acid gas, equal in bulk to the volume of the two gases before combination. Hence it follows that this gas is a compound of chlorine and hydrogen." " Muriatic acid, called hydrochloric acid by Gay Lussac, is a gaseous body, invisible and elastic like common air, and having a peculiar smell, and a very sour taste. Water absorbs it with great avidity, so that it can be preserved

* Answer, p 244. † Answer, p. 245.

only over mercury. No *combustible* body will burn in it; and it destroys life instantly, when an attempt is made to breathe it." "Its specific gravity is the mean of that of chlorine and hydrogen, or 1.2847. Hence 100 cubic inches of it weigh 39.162. grains It constituents are as follows:

Hydrogen	0.125	.....	1
Chlorine	4.5	.....	36."

The above is found at pp. 221 and 222 of Vol. 1st. In volume 2d, we have the following, at page 247.

"When equal volumes of chlorine and hydrogen gas are mixed together, if the mixture be exposed to the direct rays of the sun, or if an electric spark be made to pass through it, an explosion takes place, the two gases combine without any change of bulk, and are converted into muriatic acid gas. Hence muriatic acid is a compound of equal volumes of the two gases, or it is composed by weight of

Hydrogen	0.125	.....	1
Chlorine	4.5	.....	36."

"Since the composition of muriatic acid has become known, Gay-Lussac has thought proper to give it the name of hydrochloric acid, in order to point out its composition." "It possesses the following properties. It is invisible, like common air, and capable, like it, of indefinite contraction and expansion. Its smell is peculiar. It reddens vegetable blues, and has a very sour taste. Its specific gravity is 1.2847, that of common air being 1. Animals are incapable of breathing it; and when plunged into jars filled with it, die instantaneously, in convulsions. Neither will any combustible burn in it. If a little water be let up into a jar, filled with this gas, the whole gas disappears in an instant, the mercury ascends, fills the jar, and pushes the water to the very top. Hence the necessity of making experiments with this gas over mercury." Vol. II. p. 243 and 244.

Let us look next to Ammonia. "This substance was unknown to the ancients. The method of obtaining it, is described by Basil Valentine. If this salt (carbonate of ammonia) or sal ammoniac, be mixed with *twice its weight* of quicklime, put into a flask, and exposed to the heat of a lamp, a gas comes over, which must be received over mercury, and which is ammoniacal gas. This gas was first discovered by Dr. Priestley." "Ammoniacal gas is transparent, and colourless, and possesses the mechanical properties of air. Its smell is very pungent, though rather agreeable when sufficiently diluted. Its taste is acrid and caustic, and if drawn into the mouth, *it corrodes the skin.* Animals cannot breathe it without death. When mixed with oxygen gas, and an electric spark passed through the mixture, it detonates as was first discovered by Dr. Henry. It

converts vegetable blues into green. Its specific gravity is 0.590. Water absorbs 780 times its bulk of this gas, and is converted into liquid ammonia. When this liquid is heated to 130°, the ammonia separates in the form of gas. When electric sparks are passed for a considerable time through dry ammoniacal gas, its bulk is just *doubled*, and it is completely decomposed. The new gaseous product consists of a mixture of three volumes of hydrogen gas, and one volume of azotic gas. It is obvious from this, that ammonia is composed of three volumes of hydrogen, and one volume of azote, compressed into two volumes. Hence its constituents by weight are--

Hydrogen	0.2082	0.125×3	1
Azote	0.9722	1.75	$4\frac{2}{3}$

Thus we see that ammonia is a compound of three atoms of hydrogen and one atom of azote. Hence the weight of an atom of it is 2.125"—Vol. 1. p. 224.

"It (ammonia) may be procured in the following manner; put into a retort a mixture of *three parts* of quick-lime, and *one part* of sal ammoniac in powder. Plunge the beak of the retort below the mouth of a glass jar, filled with mercury. Apply the heat of a lamp to the retort; a gas comes over which displaces the mercury, and fills the jar. This gas is ammonia. Ammonia was altogether unknown to the ancients. Basil Valentine describes the method of obtaining it." "Dr. Priestley discovered the method of obtaining it in a state of purity. Ammonia in the state of gas, is transparent and colourless like air; its taste is acrid and caustic, like that of the fixed alkalis, but not nearly so strong, *nor does it, like them, corrode those animal bodies to which it is applied*; its smell is remarkably pungent, though not unpleasant, when sufficiently diluted. Animals cannot breathe it without death. Its specific gravity is 0.590, that of common air being 1." "Water by my trials, is capable of absorbing 780 times its bulk of this gas." "The term *ammonia* almost always means this liquid solution of ammonia in water. When heated to the temperature of 130°, the ammonia separates under the form of gas." "When oxygen and ammoniacal gases are mixed, the mixture may be fired by an electric spark, as was first observed by Dr. Henry." "Dr. Priestly discovered that when electric explosions are made to pass through this gas, its bulk is gradually augmented to THRICE the space which it formerly occupied, and a quantity of hydrogen gas is produced." "Ammonia then is composed of three volumes of hydrogen, and one volume of azote, or which comes in this case to the same thing, of three atoms hydrogen, and one atom azote. Hence its constituents by weight are:

Hydrogen	0.125×3=0.375	100
Azote	1.75	466.6

And the weight of an atom of ammonia is 2.125." Vol. II., p. 27., *et seq.* Should our readers wish for further examples of barefaced repetition, they may compare the articles, azote, carbon, phosphorus, sulphur, hydrogen, *&c.*, in the first volume, with nitric and nitrous acids, phosphoric and phosphorous acids; sulphuric acid, muriatic acid. *&c.*, in the second.

In our Review, we said, that " azote, which had figured by itself as an *incombustible* in the first volume, becomes a *combustible* in the second." Let us hear his Answer on this head. " What pitiful quibbling is the Reviewer guilty of, in order to make out the combustibility of this substance." This exclamation of the Doctor's is diverting; for the quibbling is all his own, as the following quotation proves. " The only simple *combustibles*, as far as we know at present, capable of uniting with oxygen, and forming unsalifiable oxides, are AZOTE, hydrogen, and carbon*."

Of his *consistency*, the following is a fine example.

" Chap. II. Of *simple incombustibles*. By *incombustible*, I mean a body neither capable of *undergoing* combustion, nor of *supporting* combustion. It unites to all the supporters; but the union is never attended with the evolution of heat and light. We are at present acquainted only with one such substance, namely *azote*†." What a confined intellect must that man have who contradicts himself so flatly in different parts of the same book, revised for the sixth time; and who pours out a torrent of invective on the critic, who calls azote a combustible, *on his own authority*.

" As for Dr. Wollaston," says he, " the introduction of his name is most uncandid‡." We certainly think that it was most uncandid in the Doctor to introduce such a name, in such a way; for the introduction was not our doing, but his own, as the following quotation, from his *Annals*, many months before the Review was written, will shew. " The *methods* followed by Dr. Wollaston, Professor Berzelius, Mr. Dalton, and every other person, who has hitherto turned his attention to the atomic theory, are obviously not susceptible of any great degree of precision. They have been guided entirely by the analytical researches, without any general principle to direct their choice."

In his System, he says, " All acids, therefore, (with the exception of those above-named, sulphuretted hydrogen, telluretted hydrogen, and arsenuretted hydrogen) are combinations of supporters and combustibles.||" We ventured in the Review to ask " What is iodic acid? what is chloriodic acid? what is chloric acid? what is Count Von Stadion's perchloric acid? where is their combustible element? we do not find it among

* *Thomson's System*, 6th Ed. II. 3.
† System I., p. 202.
‡ Answer, p. 251.
|| II., 80.

any or all of your combustibles.*" These questions seem to have nettled the Doctor. Let him bring order out of his own confusion at his leisure. It is not our affair.

We are glad to see his recantation, however awkward, relative to his treatment of Sir H. Davy's electrical discoveries; the merit of which, however, is too eminent to be tarnished by the Doctor's breath, and too splendid to need the varnish of *his* stile.

Dr. Thomson has nineteen pages in his System, on combustion; the first sixteen of which we again assert are absolute and unmeaning *verbiage;* and in the remaining three, he has contrived to turn into "*caput mortuum*" of his own, one of the most interesting memoirs of modern science. Yet he has the assurance to say, "I have given, I conceive, all the important additions to our knowledge of flame contained in Davy's paper." If he will look into the article *combustion* in the dictionary of chemistry, published about the same time with his sixth edition, he will find a multitude of important facts, drawn from Sir H. Davy's Memoirs on Flame, of which there is no trace in his *all-perfect* System.

Under chlorine in his System, he says of M M. Gay-Lussac and Thenard, "they shewed that the opinion, that oxymuriatic acid contains no oxygen, might be supported; but, at the same time, assigned their reasons for considering the old opinion as well founded. An abstract of these important experiments had been published however in 1809; these experiments *drew* the attention of Sir H. Davy to the subject." In our Review, the clearest evidence was adduced that Sir H. Davy's *attention* had been drawn to the subject of muriatic acid, long before these eminent French chemists had published their experiments. In his answer he perverts as usual the drift of our animadversions, to suit his private purpose. On this occasion he is driven to the necessity of citing his well-known sarcasm, against that accomplished and amiable philosopher, M. Gay-Lussac, which he vented in his Annals of Philosophy for January, 1816. It suited his temper *then*, to advocate Sir H. Davy's claims: for the controversy about the miner's safe-lamp, was yet *in embryo*.

From his answer, he appears to deny that the sun and fixed stars are masses of light, or that the cause of light is condensed in their discs; because in his System, he had affirmed of light, that "the third, and not the least singular of its peculiar properties, is, that its particles are never found cohering together, so as to form masses of any sensible magnitude." The atmosphere, electricity, and caloric, have also this *peculiar* property.

"I am accused of having perverted Davy's account of chloriodic acid, to suit my own atomic notions†." By accident, the detail of his perversion was left out of our Review. We here insert it from the original manuscript for the Dr's. edification.

* Journal of Science XI., 153. † Answer, p. 259.

"Operating in this way," says Sir H. Davy, "I find that iodine absorbs less than one third of its weight of chlorine." "In another experiment in which the sublimate was not dissolved by water, and in which the absorption was judged of, by the admission of fresh quantities of the gas, twenty grains of iodine, caused the disappearance of 9.6 cubical inches of chlorine.*" The mean of these two results gives very nearly one atom of chlorine to one atom of iodine, for the composition of chloriodic acid. Dr. Thomson omits, purposely it would appear, the statement above quoted, and brings forward another experiment, in which, from the admission of water, the same precision could not be obtained by that philosopher. He thus seeks to shew, that Sir H. Davy is in the wrong, relative to the atomic proportions, and that he himself is right, in pitching two atoms of chlorine against one of iodine.

The article *phosphoric acid* in his "Answer," contains an unfortunate anachronism which it will puzzle his veracity to reconcile. "Soon after this, I went to Glasgow (September or October, 1817), and I was not in possession of a laboratory for nearly two years. One of the first things which I did as soon as it was in my power, was to repeat Dalton's experiment (on phosphuretted hydrogen). I found it inaccurate. The whole structure immediately tumbled to the ground; and I was led back to the original opinion which I had stated in my paper on phosphuretted hydrogen gas. And those gentlemen who attended my lectures the ensuing course, will remember that I then gave the composition of phosphorous and phosphoric acids, precisely as in my sixth edition. Davy's paper appeared *soon after*, and confirmed me in the accuracy of my experiments.†" Dates are fatal to our Doctor's statement. He gives us here to understand, that nearly two years after his arrival in Glasgow, that is in the autumn of 1819, he was put in possession of a laboratory; and one of the first things he did in it, was to repeat Dalton's experiment. He adds "Davy's paper appeared soon *after*." We marvel at his venturing to publish so palpable a falsehood. For in his *Annals* for May, 1818, we have an account of Sir H. Davy's paper, on phosphorous and phosphoric acids, which had been read before the Royal Society on the preceding ninth of April.

The following is Thomson's notice, fifteen months before he was in possession of a laboratory; "In the hypophosphoric acid the proportions will be 45 phosphorus, to 15 oxygen ..... (3. : 1.0)

In phosphorous acid ..... 45 to 30 (1.5 : 1.0)

And in phosphoric acid ..... 45 to 60 (1.5 : 2.)

"On some future occasion, we hope to give a more complete

* *Phil. Trans.* for 1811, part 2d. † Answer p. 26.

account of this very valuable and elaborate paper, which we conceive to be one of the most perfect specimens of analytical research, that the author has produced*." This future occasion never arrived; but in his new edition Dr. Thomson assumes for himself the main part of the honour, which in the first moment of admiration, he here gives to the English philosopher. When he published the preceding abstract, he set up no claims of anticipation. He does not there say, as in the sixth edition of his System; "The analysis of phosphorous acid, given by Davy, in his paper published in the Philosophical Transactions for 1818, exactly agrees with mine, and serves, of course, to confirm it†." Now the Doctor's *last* decision, prior to Sir H. Davy's paper, is to be found in his fifth edition, published about six months before. Here he says, "It will appear in a subsequent part of this section, that phosphorous acid is composed of 1.5 phosphorus and two oxygen." "Thus the acids of phosphorus are composed as follows:

	Dr. Thomson. Phosph.	Oxygen."	Sir H. Davy. Phosph.	Oxygen.
Hypophosphoric acid	1.5 :	1	3 :	1
Phosphorous acid	1.5 :	2	1.5:	1
Phosphoric acid	1.5 :	3‡" ..	1.5:	2

Our readers may apply to the preceding sample of critical chicane, any epithet which they please.

With regard to the thermometer, we again affirm that his account of it is contemptible, for he gives no directions whatever, how this indispensible instrument of chemical research may be constructed, or how its indications may be verified. This is the more necessary, as the greater part of thermometers are very incorrectly made. The notice compatible with a *manual* for general students, is surely very different from what we are entitled to expect in a *system.* In lieu of printed information, the Professor invites us to attend his lectures on the subject at Glasgow, which invitation we have already given reason for declining. Indeed we had rather attend a Dutch prelection on Puffendorf.

In the number of Thomson's Annals published on the first of June, 1815, we find a copious abstract of Sir H. Davy's paper on the deutoxide of chlorine, read to the Royal Society on the preceding 4th of May. In Gilbert's *Annalen* for February following, that is eight months after Thomson's promulgation, and nine months after the above public reading, we have an account of what Dr. Thomson, in one page of his system calls "Count Von Stadion's deutoxide of chlorine, discovered *about the same time;*" but of which, he says with his usual inconsistency in the next page, "it differs so much from the gas examined by Davy, that it is probable they are distinct substances."

* *Thomson's Annals*, XI. 382. † *System*, 6th Ed. I. 263.
‡ *System*, 5th Ed. I. 266.

In adverting to this subject, the Reviewer certainly did mean to arraign the Doctor's accuracy and candour for setting up pretensions to a discovery which was probably never made, and which, at all events, from its date could not affect the prior claim of the English philosopher.

Dr. Thomson confesses his blunder, pointed out by the Reviewer, in respect to fluorine; but softens it into an arithmetical error, sinking the long train of theoretical nonsense which he had tacked to its tail. The atomic number 1.25, which he now gives for fluoric acid, is very nearly that, deducible from an experiment long ago made by Sir H. Davy, on fluate of potash. "Twenty-two grains of fused sub-carbonate of potash, decomposed by diluted liquid fluoric acid, in an experiment made with great care, was found to afford 18.15 grains of dry fluate of potash *. But 22 : 8.75 : : 18.15 : 7.22; from which subtracting the atomic weight of potash 6, we have 1.22, to represent an atom of fluoric acid.

In Dr. Thomson's Answer, we find one instance of complaisance, which we shall try to repay. "I shall gratify," says he, "this laudable curiosity of the Reviewer. I ascertained the volume of oxygen gas in the olefiant gas, by means of nitrous gas, employing Dalton's formula for the purpose †." We are surprised that so doughty a disputant, should betray the secret of his errors in so silly a way; for *Dr. Thomson, in the 6th edition of his System, condemns Mr. Dalton's formula*, which, indeed, had been shewn long ago by M. Gay-Lussac, to be good for nothing. Describing Mr. Dalton's formula, Thomson says, "To 100 measures of air, add about 36 of nitrous gas; note the diminution of bulk, and multiply it by $\frac{7}{19}$; the product gives the bulk of oxygen in the air examined. *I have made many experiments on this subject; but have* NOT *obtained results agreeing with each other.* Sometimes the multiplier is $\frac{7}{19} = 0.3684$; while sometimes it is as high as $\frac{21}{50} = 0.42$ ‡." It was the French philosopher who first cleared up by his doctrine of volumes (so strangely resisted by Mr. Dalton,) all these perplexities; thus reducing the analysis of air, or gaseous mixtures containing oxygen, to great simplicity and precision. Dr. Thomson will find a full account of this important part of practical chemistry, in the Dictionary of Chemistry, Article, EUDIOMETER.

In our Review, p. 145, his ignorance of the mode of working simple equations in algebra is exposed, though he is perpetually involving the plainest cases of arithmetic in a mist of algebraic symbols, as pedants make unlucky quotations from languages they do not understand. The merest tyro in calculation, would have been chastised at school for committing the Doctor's

* *Phil. Trans.* 1814. † *Answer*, p 267.

‡ *System*, 6th Edit. III. p. 168.

blunder; yet, on that ludicrous bottom, he rested his refutation of experiments on the constitution of carburetted hydrogen, conducted with very considerable care. "The Reviewer," says he, "has misstated my reasoning in the Annals; but it is not worth while to put him right." This statement is altogether false, as the following quotation will prove. Here is the Doctor's equation from the Annals of Philosophy.

"$x = \frac{Bb + Aa}{B + A}$

In the present case, $A = 1$. $a = 0.9722$
$B = 0.66$ $b = 0.0694$

Consequently $x = \frac{0.66 \times 0.0694 + 0.9722}{1.66} = 0.86178$."

Now we affirm that the youngest school-boy will laugh at such a shallow algebraist: for taking his own formula and numbers; $x = 0.611$ and not 0.86178. We shall be curious to see what subterfuge will next be adopted, in order to escape from this dilemma.

We shall now quote a passage from our Review, to shew the Doctor's mode of reply. "The following sentence shews the Doctor's incompetency as an experimentalist. 'Unless the flask be very completely exhausted indeed of common air, combustion takes place, when the phosphuretted hydrogen is let into it.' 'This confession betrays poverty of invention, and ignorance of the methods previously practised in such cases. If the Doctor will consult Sir H. Davy's Bakerian Lecture on the Alkaline Metals, he will find this philosopher, filling his flask with hydrogen, and then exhausting that gas, in order to get rid entirely of the atmospheric oxygen." "I affirm," says he now, "that my method is susceptible of greater accuracy than that which the Reviewer insinuates, that I did not know*." His method, which is the old one, of comparing the weight of a flask filled first with air, and then with any gas, certainly does not remove the fallacy of which he himself complained in the above quotation, arising from the partial combustion of the phosphuretted hydrogen, by the residuary air; and, indeed, since *his* air-pump always leaves a notable residuum of common air in his flask, "his method" is totally inapplicable to phosphuretted hydrogen. We would advise him to fill his flask after the first exhaustion, with azote, and not till after a second, or, perhaps, a third operation, to admit the combustible gas.

But the boasting tone in which he talks of his superior skill in taking the specific gravity of gases, is finely contrasted with his total ignorance of the subject. Thus Dr. Thomson speaks of himself as a person "who has determined the specific gravity

* *Answer*, p. 263.

of more than 20 gases, with a degree of care and accuracy seldom equalled, *and never surpassed* *."

Dr. Apjohn in his " remarks on the influence of moisture in modifying the specific gravity of the gases," inserted in the number of the Annals, for May, 1822, justly says of Dr. Thomson, " his mode of estimating the effect of moisture on the densities of the gases, appears to me altogether incorrect. The principle of the method adopted by Dr. Ure for the same purpose is true, but his number for steam being too high, his results are erroneous." Dr. Ure's number for the specific gravity of the vapour of water at 212°, is 0.625, and 0.624 is the number given by M. Biot, in his Traité de Physique, on the authority of M. Gay-Lussac†. Nothing is there said of temperature, though it appears that M. Gay-Lussac in his valuable researches, did give the relative densities of steam and air, for the boiling point of water at $\frac{10}{16}$, or 0.625. This is also the theoretical quantity, regarding two volumes of vapour of water, as consisting of two volumes of hydrogen 0.13888

1 of oxygen 1.11111

1.24999

one half of which sum = 0.625, is the specific gravity, air at 212° being 1.000. But as 1.375 parts of air at 212° become 1,05833 at 60°, we shall have the density of steam at 212° to that of air at 60° = $0.625 \times \frac{1.05833}{1.375} = 0.481$ and not 0.472, as adopted by Dr. Apjohn from Dr. Thomson.

We shall now give a tabular view of the different results.

Gases.	Sp. Gr. of Dry.	Specific Gravity of Moist. Thomson‡.	Ure§.	Apjohn.
Air,	1.0000 . .	0.9829	0.9934	0.9907
Oxygen,	1.1111 . .	1.0921	1.1020	1.0998
Azote,	0.9722 . .	0.9555	0·9660	0.9634
Chlorine,	2.5000 . .	2.4583	2.4677	2.4644
Hydrogen,	0.0694 . .	0.6839	0.0789	0.0761

Ure's and Apjohn's numbers do not differ much. Thomson's are ridiculously erroneous, and would vitiate every investigation on gaseous matter. The true quantities, computed from Ure's rule, are as follows:

Specific gravity of Gases standing over water at 60°.

Air	0.9909
Oxygen, ...	1.0995
Azote, ...	0.9636
Chlorine	2.4653
Hydrogen, ...	0.07648

* *Answer*, p. 268.

† *Tableau de la pesanteur specifique des gaz, et de quelques vapeurs.* Vol. I. p. 383.

‡ These quantities are taken, either from his late paper on the moisture of the gases, or computed from his formula.

§ Deduced from the numbers given at the end of the article, Gas —*Dictionary of Chemistry.*

"The ratio, therefore," says Dr. Apjohn, "of the dry gas to the expansion produced in it, by the moisture, is that of $\frac{p-f}{f}$: 1. Dr. Thomson, through some oversight, considers this as the ratio in volume of the dry gas to the *steam*, with which it is saturated, whereas the true ratio of these is that of $\frac{p-f}{p}$: 1; inasmuch as a given volume of gas at any temperature saturated with moisture, contains as much steam as could exist at that temperature in a vacuum of equal capacity*." In numbers, the first ratio is that of 0.983 to 1; the second is 56.7 to 1; and 0.983 : 56.7 :: 1 : 57.7; so that Thomson commits the *trifling* blunder, on a subject in which he conceives himself supreme master and proficient, of making the volume of vapour in a moist gas about $\frac{1}{60}$ of the true quantity.

"We see from these examples," says Thomson in this wonderful paper "*on the influence of humidity in modifying the specific gravity of the gases*," "that the specific gravity of air is diminished very nearly, by the volume of vapour mixed with it. And the *lower the temperature, the more nearly does this approach to accuracy;* because the specific gravity becomes always *less and less considerable*." What ignorance of physics! "The densities of all the rest (except hydrogen,)" says Dr. Apjohn, are diminished by moisture, and the *more* as we *descend* on the scale of temperature; for as we descend, their specific gravities increase, and of course the difference between steam and the constant quantity 0.472. As an inference also from this result, I may remark, *that the rule so much insisted upon* of taking the specific gravities of the gases at a low temperature, is so far from being general, as to apply to hydrogen alone †." Now listen to the regius expounder of science. "I have little doubt that the specific gravity of hydrogen gas found by Berzelius and Dulong; namely, 0.0688, was a little too light, in consequence of the presence of the vapour of water in it ‡." This is much too absurd. The vapour of water is seven times denser than that of hydrogen, and must therefore always increase the specific gravity of that gas. What a pity that Dr. Apjohn published his exposure of the Doctor's notions so early. We should have been favoured ere long from his laboratory, with experimental determinations of the specific gravities of the gases, saturated with moisture, in which the results would all have been clipped and trimmed to his fantastic tabular numbers, like the grotesque shrubs in a Dutch garden.

* *Annals of Phil.* May 1822. p. 386. † *Annals of Phil.* p. 387.
‡ *Annals of Phil.* April, 1822.

Of sheer effrontery, the following is a good example. He is trying to palliate the absurdity of his claims, relative to sulphurous acid. " As for the experiments of Davy to which the Reviewer refers, as the first which established the true composition of sulphurous acid, I am not acquainted with them * " Not acquainted with them! They are quoted in the Review from pages 273 and 274, of the Elements of Chemical Philosophy. We shall here re-quote the passage for Dr. Thomson's information. " If the specific gravities of sulphurous acid gas and oxygen be compared, and the last subtracted from the first, it will appear that sulphurous acid consists nearly of equal parts of oxygen and sulphur by weight. In *several experiments in which I burned sulphur, procured from iron pyrites, out of the contact of air or moisture, in dry oxygen over mercury, I found that the volume of the oxygen was very little altered.*" " These are experiments," adds the Reviewer, " in which the world may confide. Dr. Thomson sinks them entirely, and refers merely to his own, on which nobody can rely †.

Under lithia, we find him re-asserting that 2.25, is the equivalent number deducible from the mean of the foreign experiments. Now, according to Gmelin, 100 parts of carbonate of lithia consist of 45.54 lithia, and 54.46 acid. But 54.46 : 275 : : 45.54 : 2.299, or 2.3 as we stated in the Review. Again, M. Vauquelin says, " that 100 parts of lithia contain 43.5 of oxygen, and 56.5 of metallic base; and 43.5 : 1 : : 56.5 : 1.299 for the atom of lithium, to which, adding 1 for oxygen, we have that of lithia = 2.299 as above.

He even defends his absurd method of preparing muriate of barytes, by pouring muriatic acid on the calcined sulphuret, instead of adding it to its filtered solution, on the score that the muriatic acid of commerce is never free from iron. Chemists who operate with pure acids (and the muriatic need never be contaminated with iron,) will do well not to vitiate their product, by following Dr. Thomson's mis-direction. We can readily understand why he affects to prefer Mr. Dalton's table of sulphuric acid, which at the specific gravity 1.8447 is, by his own shewing, $4\frac{1}{2}$ *per cent.* wrong, to Dr. Ure's, which, at the above density, must be right, since it agrees with his own number, and which, in the other points, is probably never in error to the amount of one-half per cent.

The Doctor is peculiarly incensed at our exposure of the bundle of errors in his paper on oxalic acid, inserted in the Philosophical Transactions for 1808. We shall quote the pas-

* *Answer*, p. 268. † *Review*, p. 149.

sage, as it is a good sample of his way of tempting readers into a belief that he is right, while he must know that he is wrong. "The allegation of the Reviewer, that my analysis of oxalic acid was inaccurate, because my oxalic acid still retained 32.5 *per cent.* of water, is disgracefully unjust. He must have known that my experiments were not made upon oxalic acid, but upon a dry oxalate. *No water existed in the acid as I employed it*, and therefore none was to be deducted. My near approach to truth in these experiments, notwithstanding the numerous difficulties attending my method, affords unequivocal evidence of the great care employed in the experiment *." The words we have put into Italics are false, and he knows them to be so, as the world shall also forthwith learn.

"Eighty-nine grains of well-dried oxalate of lime," says Dr. Thomson in the above memoir, "were exposed in a small retort, to a heat gradually raised to redness; the products were the following:—

	Grains
45.6 cubic inches of gas, weighing . .	14.8
WATER	6.4
Residue in retort	62.4
	83.6
Loss	5.4
Total . .	89.0

"The 62.4 grains of residue in the retort were composed of

Lime	33.4 grains
Carbonic acid .	26.4
Charcoal	2.6
	62.4

"Now it is clear that the 89 grains of oxalate of lime were composed of lime 33.4, acid 55.6 = 89. The acid was completely decomposed, and resolved into the following products:

Carbonic acid	33.1
Inflammable air	13.5
WATER	6.4
Charcoal	2.6
	55.6

"Had the experiments been made upon 100 grains of oxalic acid, instead of 55.6, it is clear that the proportions would have been as follows:

Carbonic acid	59.53
Inflammable air	24.28
WATER	11.51
Charcoal	4.68
	100.00

* *Answer*, p. 271.

"The result of the other experiments on oxalate of lime were nearly the same as the preceding. The following may be stated in round numbers, as the mean of the whole:

Oxalic acid is a compound of	Oxygen	64
	Carbon	32
	Hydrogen	4
		100*"

We shall now shew that more errors are crowded into the above concluding statement of the Doctor's paper, than are to be found in any dozen volumes of the Royal Society's Transactions besides. It is now known from the concurring results of Dulong, Berzelius, and Dobereiner, to which some decisive ones obtained in this country will soon be added, that oxalic acid, such as it exists in oxalate of lead, contains no hydrogen †. Now as Dr. Thomson's contained 4 per cent. of hydrogen, we must allow it 32 of oxygen, being the quantity equivalent to its saturation, and of course 36 per cent. of water is the inevitable result—a quantity actually greater than the Reviewer assigned. Though he says in his answer, "*No water existed in the acid as I employed it,*" yet we find in the very memoir under discussion $11\frac{1}{2}$ per cent. of water *openly* stated. But oxalate of lime is by no means necessarily an anhydrous compound. "L'oxalate de chaux," says Berzelius in his late memoir, "peut se combiner avec de l'eau en deux proportions, dont l'une est $C\,a\,O^2 + 2\,aq.$ and the other $C\,a\,O^2 + 4\,aq.$, et le plus souvent on les obtient melées ensemble‡."

The above anhydrous oxalate is 8.0, to which adding 2 atoms water = 2.25, we have the sum 10.25; whence such a salt contains in 100 parts 22 of water. Thomson's contained 4 of hydrogen, along with its saturating quantity of oxygen, constituting a sum = 36 *per cent.* Now this is exactly the quantity corresponding to 4 atoms of water; for 8 oxalate + 4.5 water = 12.5, and 12.5 : 4.5 :: 100 : 36. Now taking away these 36 parts of water, or 4 hydrogen and 32 oxygen, certainly foreign to the constitution of real oxalic acid, we have a remainder of 32 carbon, and 32 oxygen, for its composition as given in the above blundering memoir. This proportion transferred to atomic numbers is 3 atoms of oxygen to 4 atoms of carbon; whereas its true composition is 3 atoms of oxygen to 2 atoms of carbon. *Such was his near approach to truth*, or rather such was his affinity to error *in these experiments!* He makes the proportion per cent. of carbon to be 50, while it is only 33.3. "Now it is clear," said he, "that 89 grains of oxalate of lime, well dried, were composed of lime 33.4, acid 55.6." From the sixth edition of his System, "*it is clear*" that 89 grains of well-dried oxalate of lime are composed of lime 39 . acid 61. For 8.0 : 3.5 :: 89 : 39; or rather

* Phil. Trans. for 1807, p. 63.

† See Berzelius's paper on the muriates of platinum and gold. *Annales de Chimie et de Physique*, Oct. 1821.

‡ *Ibid.* p. 156.

38.9375. So erroneous is he in this most simple matter! In fine, nothing is clear from this memoir, except the elaborate stupidity of its author.

In the answer we have three vain pages on mineralogy, but Dr. Thomson is careful to suppress the *private* reason, why Professor Jameson's name is erased from his references at the bottom of the pages of his sixth edition, while the text is retained; why every reference to the Scotish Mineralogist is expunged; and why, though in the sixth edition of the System, he has retained, most servilely, the long chapter "Of Compound Rocks," amounting to 35 pages, borrowed in his fifth from Professor Jameson's Geonosy, he has now obliterated the following note acknowledging the obligation! "For to that important work (Jameson's Elements of Geonosy,) I earnestly recommend the attention of every mineralogist. The sketch in the text, though only a very short abridgment, is as detailed as is consistent with the nature of the present work, *I was indebted to Professor Jameson for the* WHOLE *of the materials out of which it was formed.*"

Under "Analysis of Minerals" in his "Answer," he says, "The Reviewer has pointed out one or two typographical errors, and invented as many more." This is a palpable falsehood. The blunders pointed out by the Reviewer, in Thomson's Chapter on the Analysis of Minerals, arise not from the carelessness of the printer, but the habitual inaccuracy of the author, which is here so redundant as to leave no room for invention on the Reviewer's part.

His assertion about liquid muriatic acid we had nearly passed over. We affirm that such experimental results as he gives, were never obtained; for they are impossible; and if he knew any thing of Chemical Arithmetic, he would not have offered them to the public. The demonstration is given in the Review, as clearly as numbers can make it. If he wants more evidence on this subject, he will find it in the last number but one, of this Journal.

With what decency or prudence can Dr. Thomson speak of the severity of our censure of Berzelius' mineralogy in an early number of this Journal, when *he himself* was the sole cause of that severity, by ushering before the English public a pretended translation of the Swedish work, so distorted and erroneous as to excite the grief and indignation of its celebrated author, and to lead him to lay a formal charge of something like literary fraud against Dr. Thomson, before the tribunal of European science *!

We have taken some pains to ascertain how far Dr. Thomson is correct in what he says, pages 253 and 254 of his *Answer*,

* See the complaint of Berzelius, in our Review of his Treatise on the Blow-Pipe, page 319 of this Number.

concerning Sir H. Davy and the atomic theory; and we are warranted in affirming, that his account of the matter is altogether false. Sir H. Davy has never embraced the atomic theory as taught by Mr. Dalton, or as expounded by the Regius Professor. With the first and great part of the doctrine of chemical proportions, that of reciprocal saturation of acids, bases, and salts, Sir H. was acquainted long before Mr. Dalton broached it. It was clearly taught by Richter; and a fine example of it is to be found in the Chemical Researches on Nitrous Oxide, bearing date, June 25, 1800. We know, at the present day, of no single specimen of chemical equivalents bletter developed than the following: "Hence," says Sir H., "we gain the following equation:—

"6.5 green sulph. of iron = 2.41 sulph. acid + 4.1 gr. ox. iron.
1.44 nitrous gas = 0.64 nitrogen + .8 oxygen.
1.0 water = 0.85 oxygen + 0.15 hydrogen.
Equal,
3.2 sulph. ammon. = 2.41 sulph. acid + 0.64 nitrog. + 0.15 hydr.
5.7 red ox. iron = 4.1 gr. ox. iron + 1.6 oxygen †."

Was the author of this equation, and of a multitude of similar researches, published so early as 1800, to get his first glimpse of the *atomic theory*, as Thomson miscalls it, or of the theory of reciprocal proportions, from any long conversation with Dr. Thomson, in 1807? How dare the Dr. say, therefore, "I knew from Davy himself that he had no idea whatever of definite proportions till Mr. Dalton had made known the outlines of his theory*." Mr. Dalton, through the courtesy of Sir H. Davy and his friends, was brought forward to lecture in the R. I. in 1803; at which time this ingenious observer thought he saw in Sir H. Davy's researches on the compounds of azote and oxygen, indications of multiple proportions, an idea manifestly thrown out and abandoned to the world by Mr. Higgins, a few years before. Sir H. Davy, with that integrity of intellect and attachment to the Baconian logic, which distinguishes all his inquiries, thought that Mr. Dalton strained experimental results to suit his theoretical opinions, and therefore hesitated to adopt the second part of what is now called the atomic theory, namely, the doctrine of multiple proportions, till experiments should demonstrate its truth. Mr. Dalton persevered most laudably to make out, by an extensive induction of facts, the justness of these views; which received their final confirmation from Dr. Wollaston's paper on the super-oxalic and sub-oxalic salts, and M. Gay-Lussac's beautiful memoir on gaseous volumes. At present, the theory of reciprocal and multiple proportions, as taught in Sir H. Davy's Elements of 1812, is undoubtedly a much truer representation of chemical principles than any thing

* Researches concerning nitrous oxide, p. 172.
† Answer, p. 254.

which Dr. Thomson has ever written, or can ever write on the subject.

We have now accompanied the Doctor through the various turnings of his tortuous course, and our readers must perceive how heavily his Answer has fallen back upon himself. Many of our most important criticisms he prudently shrunk from; but as these are fully before the public, we shall not fatigue our readers with references and repetitions.

Had Dr. Thomson confined his "Answer" to the statements contained in our "Review;" had he endeavoured, as he should have done, to have vindicated his character as a systematic writer by a calm and temperate reply, showing that our charges were exaggerated, and our conclusions erroneous, we should have left him in the quiet enjoyment of such refutation; but the tone and tenor of his answer are such as amply to confirm the justice of our criticism, and to seal his own condemnation; it is overbearing, petulant, and personal—full of topics entirely irrelevant, and evidently brought in with a view of drawing the reader's attention to any thing but the subject of our animadversions. In language much stronger than we should have any where used, Dr. Thomson tells his readers that we have accused him of being utterly incapable of writing English; of being ignorant of the first principles of arrangement; of having made many false statements to promote his own absurd and erroneous chemical speculations; and of having stuffed his book with innumerable errors, arising from his want of acquaintance with the elements of chemical science. These, we repeat, are Dr. Thomson's words; how far they are *true*, we now leave our readers to determine.

ART. XII.

ASTRONOMICAL AND NAUTICAL COLLECTIONS.

No. X.

i. *A Table to facilitate the calculation of the Equation to equal Altitudes, by* D. Josef Sanches Cerquero, *Lieutenant of a Frigate of the National Navy, in the Observatory of the City of Saint Ferdinand. Translated and communicated by* Mr. Andrew Livingstone.

Call the hour shown by the chronometer at the time of the first observation in the forenoon H, the corresponding apparent time h, the time which the chronometer is fast of apparent time a, the horary angle in parts of the equator P; and we shall have

$$H = h + a$$

$$h = 24^h - \frac{1}{15} P.$$

And consequently

$$H = 24^h + a - \frac{1}{15} P. \quad (A)$$

For the corresponding observation in the afternoon, indicating the quantities by the same letters, but with accents to distinguish them, and adding 24^h to H', that both observations may reckon from the same time, we shall have

$$24^h + H' = h' + a'$$

$$h' = \frac{1}{15} P'$$

And therefore

$$24^h + H' = a' + \frac{1}{15} P'. \quad (B)$$

The equations (A) and (B) give

$$\tfrac{1}{2}(a + a') = \tfrac{1}{2}(H + H') - \frac{\tfrac{1}{2}(P - P)}{15}. \quad (C)$$

The quantity $\frac{1}{2}(a + a)$ is what the chronometer is *fast* of the precise middle time of the two observations, which is nearly the time of apparent noon; and the equation (C) shows that what it is fast is equal to the half sum of the two times H and H', *plus* a correction equal to half the difference of the two horary angles reduced to time.

The chronometer has been supposed *fast* of apparent time, and this may always be considered as the case, for if *slow* it can easily be read *fast* by merely subtracting what it is slow from 12^h.

Spherical Trigonometry gives

$$\tfrac{1}{2}(P' - P) = \frac{\tfrac{1}{2} x}{\text{sine } \tfrac{1}{2}(P' - P)}. \quad \text{Tang. } L.$$

$$- \frac{\tfrac{1}{2} x}{\text{tang. } \tfrac{1}{2}(P' + P)}. \quad \text{Tang. } D.$$

x being the sun's declinatory movement towards the North Pole in the interval betwixt the observations, L the latitude of the place, supposed North, and D the declination of the sun, also North, calculated for a time equidistant from both observations, which in the present case is the declination for apparent noon.

In this value of $\frac{1}{2}(P' - P)$, the quantities of the third order only have been neglected, being absolutely insensible, as has been shown by DELAMBRE in his *Treatise on Astronomy*.

If the interval expressed in hours is called T, it is easy to see that $$15^\circ\, T = P + P'.$$

If the diurnal movement in declination is expressed by d D, it also follows that

$$24^h : T :: \mathrm{d}D : x = \frac{T}{24}.\ \mathrm{d}D$$

Substituting these values in the expression of $\frac{1}{2}(P' - P)$ and dividing by 15 to reduce it to time, it results that

$$\frac{\frac{1}{2}(P' - P)}{15} = \frac{\frac{1}{2}T}{360.\ \mathrm{Sine}\left(15^\circ \frac{T}{2}\right)}.\ \mathrm{d}D\ \mathrm{Tang.\ L}$$

$$- \frac{\frac{1}{2}T}{360.\ \mathrm{Tang.}\left(15^\circ \frac{T}{2}\right)}.\ \mathrm{d}D\ \mathrm{Tang.}\ D$$

$$= A\,\mathrm{d}D\ \mathrm{Tang.\ L} - B\,\mathrm{d}D\ \mathrm{Tang.\ D}$$

And in the last place, substituting in the equation (C)

The hour of apparent time by the chronometer $= \frac{1}{2}(H + H)$
$- A\,\mathrm{d}D\ \mathrm{Tang.}\ L$
$+ B\,\mathrm{d}D\ \mathrm{Tang.}\ D$

The two last terms jointly form what is denominated the *equation to equal altitudes.*

If another corresponding altitude is observed the subsequent forenoon, and designated by the same letters, but with two accents, to distinguish the quantities appertaining to this third observation, we shall have

$$24^h + H' = a' + \frac{1}{15}P'$$

$$H'' = 24^h + a'' - \frac{1}{15}P''$$

And consequently

$$\frac{1}{2}(a' + a'') = \frac{1}{2}(H' + H'') + \frac{\frac{1}{2}(P'' - P')}{15}.\quad (E)$$

The quantity $\frac{1}{2}(a' + a'')$ will be the time that the chronometer was *fast* at midnight, or what is the same, the time of apparent midnight by the chronometer.

We shall also have in this case

$$\frac{1}{2}(P'' - P') = \frac{\frac{1}{2}x}{\mathrm{Sine}\ \frac{1}{2}(P' + P'')}.\ \mathrm{Tang.}\ L$$

$$- \frac{\frac{1}{2}x'}{\mathrm{Tang.}\ \frac{1}{2}(P' + P'')}.\ \mathrm{Tang.}\ D'$$

x' being the variation of the declination from the time of the afternoon until the time of the observation of the following forenoon, and D' the declination for midnight, or the time equidistant from both observations; calling the interval in time T, we have as before

$$x' = \frac{T}{24} \cdot \mathrm{d}D$$

$$(P' + P'') = 15^\circ\,(24 - T')$$

$$\tfrac{1}{2}\,(P' + P'') = 180^\circ - \left(15\,\frac{T'}{2}\right).$$

Substituting these values in the expression of $\frac{1}{2}\,(P'' - P')$, keeping in mind that

$$\text{Sine}\,(180^\circ - M) = \text{Sine}\,M$$

$$\text{Tang.}\,(180^\circ - M) = -\,\text{Tang.}\,M,$$

and dividing by 15 to reduce it to time, it follows that

$$\frac{\frac{1}{2}\,(P'' - P')}{15} = \frac{\frac{1}{2}\,T'}{360\,\text{Sine}\,\left(15^\circ\,\frac{T}{2}\right)}\,\mathrm{d}D\,\text{Tang.}\,L$$

$$+\,\frac{\frac{1}{2}\,T'}{360\,\text{Tang.}\,\left(15^\circ\,\frac{T'}{2}\right)}\,\mathrm{d}D\,\text{Tang.}\,D'$$

$$= A'\mathrm{d}D\,\text{Tang.}\,L + B'\mathrm{d}D\,\text{Tang.}\,D'$$

Finally substituting in the equation (E)

Time of apparent midnight by the chronometer $= \frac{1}{2}\,(H' + H'')$
$+\,A\,\mathrm{d}D\,\text{Tang.}\,L$
$+\,B\mathrm{d}D\,\text{Tang.}\,D'$

Comparing this expression with that which we have before found for noon, we see that they only differ in the sign of the first term of the correction. And thus, taking altitudes of the sun in the afternoon, and corresponding ones the following forenoon, we can find the time of midnight by the chronometer with equal facility, and by the same calculation as for noon, only changing the sign of the first term of the correction.

The interval T or T' ought strictly to be computed in apparent time; but good watches go so accurately, that 24 hours by the watch will be so nearly 24 hours of apparent time, that the interval shown by the watch may be considered as the interval of apparent time, without the least risque of any sensible error.

The table hereafter inserted gives the Logarithms of A and B to four places of figures; it is of single entry, the argument being the interval, "or elapsed time:" and for the whole calculation of the equation Logarithms to only four places of figures suffice.

Log. A + Log. d D + Log. Tang. L = Log. of the first part;

Log. B + Log. d D + Log. Tang. D = Log. of the second part.

Thus we see the whole operation reduced to finding the Logarithms A and B by the table, and three others to four places of figures by the ordinary Logarithmic Tables, of which every observer must be possessed.

In relation to the signs, the factor A is positive if the correction is required for midnight, and negative when it is wanted for noon: B is negative when the interval exceeds 12 hours; d D is negative when the declinatory movement is towards the south; Tang. D is negative if the declination is south; as is also Tang. L when the observer is in a southern latitude. On the right of each logarithm may be written the sign of the factor it represents, in the form shown in the examples.

With what has been said, and giving to each part the sign which results from the general rules of multiplication, the correction is obtained with more facility and accuracy united than by any other mode with which I am acquainted: in fact, the calculation is very short, symmetrical for both parts, the result exact to the tenth of a second in all cases which offer in practice, and answers for either the sun or the planets.

The declination of the sun must be taken from the *Nautical Almanac*, for noon or midnight, as may be required; but that of a planet, for the time of its superior or inferior passage.

Some may consider the attention to the *signs* of the factors necessary to show that of the product as troublesome, desiring some particular rule as better for assigning the manner in which each part of the equation ought to be applied. Persons of so bad a taste may avail themselves of the following rules when solar altitudes are observed; which will most frequently be the case.

The first part is additive when the sun's longitude is greater than 3^s and less than 9^s, otherwise it is subtractive. But the

reverse takes place in south latitude, or if the correction is required for midnight, unless both these circumstances concur.

The second part is additive when the sun's longitude is comprehended between 0^s and 3^s, or between 6^s and 9^s; otherwise subtractive. But this application changes if the interval exceeds 12 hours.

Example. On the afternoon of the 17th of September, 1810, altitudes of the sun were observed at Marseilles; and equal ones on the forenoon of the 18th: the interval was $21^h\ 50^m$; the latitude of Marseilles 43° 17′ 50″ N.: the "*Connaissance des Tems*," gives the sun's declination for midnight of the 17th at Marseilles as 2° 14′ 23″ N.; the diurnal declinatory movement d D being $-$ 23′ 14″ $=$ $-$ 1394″. With these data the equation to equal altitudes is required.

Log. A (Table)	9.0349 +	
Log. Tang. L	9.9742 +	
Log. 1394″	3.1443 —	
Log of 1st part	2.1534 —	142″,37 first part.
Log. B (Table)	9.0172 —	
Log. Tang. D	8.5923 +	
Log. 1394″	3.1443 —	
Log. of 2d part	0.7538 +	5,″67 second part.

Equation for midnight — 136″,70 $=$ $2^m\ 16^s$,70.

Former tables which give the equation for noon, and which are to be found in Lalande's Astronomy, Mendoza's Collection, and several of the Spanish Nautical Almanacs, have all of them the following defects:—1st, they are of double entry, and consequently both complicated and troublesome in use, exacting double, and even triple, proportional parts: 2d, they are constructed for short intervals, and consequently "in the majority of cases" cannot give the correction for midnight: 3d, they suppose the perihelium of the earth, and the obliquity of the ecliptic, constant, and it is necessary if accuracy is required to renew them from time to time: 4th, they are of no use for planets.

Baron Zach, in 1812, published at Marseilles, at the end of his particular tables of aberration and nutation, others for the present object which we are engaged in; they are free from the 1st and 2d objections, but the 3d and 4th still remain in his

XIXth table. To deduce the equation of Altitudes from these tables, the author finds two auxiliary arcs α and β, and two numbers a and b; and afterwards has to seek five logarithms to seven places of figures, and even then in the preceding example which he has computed in his introduction, he does not arrive at the exactitude of the tenth of a second. In fact, he himself says, that using his table XIX, he found "for the correction $-2^m\ 16^s,77$."

It is true he proposes another method accurate in its results, and applies it himself to the same example; but by this mode the same two auxiliary arcs α and β must be found, and also seven different logarithms to seven places of figures, "although two of them are constant," to obtain the solution $-2^m\ 16^s,67$, which is the same we have found with so much greater facility with merely the unimportant difference of $0^s,03$.

On this occasion we ought to notice some ambiguities which Baron Zach has overlooked in the application of his tables to the correction of the transit of a planet, determined by means of equal altitudes. At Florence, he says, Mars was observed on 8th April, 1809; the interval being $= 8^h\ 20^m$; $dD = +\ 6'\ 38''$; latitude $= 43°\ 46'\ 40''$ N.; declination $= 5°\ 9'\ 40''$ S; required the correction for the planet's superior passage?

By our method we shall have

Log. A (Table)	8.1155	−	
Log. Tang. L	9.9815	+	
Log. 398″	2.5999	+	
Log. of 1st part	0.6969	—	4″,98 first part.
Log. B (Table)	7.7799	+	
Log. Tang. D	8.9558	−	
Log. 398″	2.5999	+	
Log. of 2d part	9.3356	−	0″,22 second part

Equation − 5″,20

Baron Zach finds, First part + 4″,97
Second part − 0″,22

Equation . . . . + 4″,75

Re-examining his calculation (page 36 of the prefatory part of his little work already cited) and correcting the slight oversight in taking out the number corresponding to the Log. 0.6968897, we find for the first part + 4″,98. Correcting also

for having given the sign + to the Tang. of the declination in the calculation of the second part when it ought to bear the sign — in consequence of the declination being *South*, we find or the second part + 0″,22, and the total equation + 5″,20, the same that we have deduced above, but with the contrary sign; it thus appears that independently of the oversight peculiar to this example, Baron Zach has changed the sign of the whole equation.

To make this more perceptible, let us calculate the other example proposed in the same page. At Pisa equal altitudes of Venus were taken on the 23d of March, 1809; the interval being = $8^h\ 50^m$; latitude 43° 43′ 11″ N., declination = 20° 42′ 40″ N.; and d D = + 20′ 5″, or 1205″.

Log. A (Table)	8.1272 —	
Log. Tang. L	9.9806 +	
Log. 1205′	3.0810 +	
Log. 1st part	1.1888 —	15″,45 First part.
Log. B (Table)	7.7322 +	
Log. Tang. D	9.5776 +	
Log. 1205″	3.0810 +	
Log. 2d part	0.3908 +	2″,46 Second part.

Equation . . . — 12″,99
According to Zach + 12″,98

It is thus seen that using Zach's rules the sign is equivocal for the passage of planets; this, in my opinion, arises from that celebrated Astronomer's having changed (without doubt from inattention) the fundamental formula for the correction of the superior passage. In effect that formula is as we have already seen.

$$\text{Equation of altitudes} = -\frac{\frac{1}{2}T}{\text{Sine}\left(15^\circ \frac{T}{2}\right)} \cdot \frac{1}{360} \cdot \mathrm{d}D \text{ Tang. } L$$

$$+ \frac{\frac{1}{2}T}{\text{Tang.}\left(15^\circ \frac{T}{2}\right)} \cdot \frac{1}{360} \cdot \mathrm{d}D \text{ Tang. } D$$

Zach exhibits this in his work before mentioned (page 30,) with the signs reversed.

Mr. Delambre has also mistaken the signs of the analytical expression of the equation for midnight, or the inferior passage, in

his *Compendium of Astronomy*, (page 223,) and in his *Astronomy*, (vol. 1st, page 560.) This may easily be seen by comparing the said expression as found in the two Treatises just cited, with what we have above deduced, or with that which the author himself has stated to be correct in the prefatory part of his Solar tables, published by the (French) Board of Longitude in 1806.

Such mistakes, and made by such respectable authors, have obliged me, at the beginning of this memoir, to occupy myself in giving demonstrations of both formulas, in order to show, beyond a doubt, which are the correct expressions.

The same Mr. Delambre, in the first volume of his Astronomy, has given tables for the equation of altitudes; by which five logarithms are to be found, as in our method, with only this difference that four of them are found in his tables; it is however necessary to have recourse to the ordinary logarithmic tables for the tangent of the latitude; and to find the two parts of the equation by means of his logarithms: besides, four proportional parts are almost always required for the four logarithms which his tables furnish, while by our method no proportionals are required; "except, perhaps, for the logarithms A and B:" and in finding the log. tangents of the latitude and declination, the nearest minute may be used, without danger of any sensible error in the result, from neglecting the odd seconds. I believe, therefore, that if no disadvantage attends Delambre's mode, it at least has no advantage over our method in point of brevity. In regard to accuracy, the 1st table has the 3d and 4th defects of the general ones of double entry which have hitherto been in use, defects common to all the class of which the sun's longitude is the argument. In the case of the first example we have given, with the said longitude for midnight of the 17th, at Marseilles, (174° 22′) the tables we speak of give $2^m\ 16^s,9$ for the equation of equal altitudes, differing from that found above by Zach's accurate method, and by our own, by $0^s,2$; a quantity which Astronomers will not disregard without some reluctance; to this may be added that the differences in the said 1st table are considerable in some cases, and the interpolation is consequently very troublesome.

Those who, nevertheless, prefer Delambre's table, ought to

keep in mind that there is a mistake of the sign in the 1st table, page 578, column 4th. The whole of that column which gives the log. $\left(\frac{\mathrm{d}\,D \text{ tang. } D}{360}\right)$ from 180° to 225° of the sun's longitude ought to have the sign + in place of − which has there been assigned to it either through the inattention of the celebrated author, or from an error of the press. There is also another mistake in the 2d table in the log. $\left(\frac{t}{\text{tang. } (15^\circ\ t)}\right)$ corresponding to $8^h\ 20^m$ of half-intervals, and which ought to be 0.7660 instead of 0.7460.

Finally, we cannot do less than recommend to astronomers and travellers, who are desirous of regulating their timekeepers by equal altitudes taken on two or more consecutive days, the advantage which results, without any increase of trouble, from making all the observations corresponding ones; that is, if in the forenoon and afternoon of the same day equal altitudes are obtained, it is very desirable that those of the subsequent day should not only be equal between themselves, but also equal to those of the preceding day; and also that the same should be the case on the subsequent days. Proceeding on with this care, the time of noon and midnight may be accurately found, and consequently the exact rate of the chronometer from 12 to 12 hours, thus establishing confidence in any observation made by that timekeeper during the interval from one *rating* to another.

It is also useful to take altitudes in the afternoon when clouds have prevented them from being observed the preceding forenoon, and with these and corresponding ones next forenoon the chronometer's absolute rate for midnight may be found; this is still more requisite if any observation is to be made by that chronometer during the same night, for it is interesting to obtain a rate for the timekeeper which will assure the apparent or mean time as near as possible to the time as shown by it when any phenomenon takes place.

It appears that many foreign Astronomers practise this method, and thus my advice is principally addressed to my countrymen, among whom I have seen none use it, notwithstanding its facility and utility.

A TABLE, intended to facilitate the calculation of the EQUATION to equal Altitudes. Argument = T = interval.

Arg.	Log. A ∓	Diff.	Log. B +	Diff.	Arg.	Log. A ∓	Diff.	Log. B +	Diff.
h m					h m				
2 00	8.0307	+ 9	8.0157	− 18	7 00	8.0883	+ 31	7.8728	− 95
10	8.0316	9	8.0139	20	10	8.0914	32	7.8633	101
20	8.0325	10	8.0119	21	20	8.0946	33	7.8532	106
30	8.0335	11	8.0098	22	30	8.0979	34	7.8426	112
40	8.0346	11	8.0076	24	40	8.1013	34	7.8314	118
50	8.0357		8.0052		50	8.1047		7.8196	
		12		26			35		125
3 00	8.0369	13	8.0026	28	8 00	8.1082	36	7.8071	132
10	8.0382	14	7.9998	29	10	8.1118	37	7.7939	140
20	8.0396	14	7.9969	31	20	8.1155	38	7.7799	149
30	8.0410	15	7.9938	33	30	8.1193	39	7.7650	159
40	8.0425	16	7.9905	35	40	8.1232	40	7.7491	169
50	8.0441		7.9870		50	8.1272		7,7322	
		17		37			41		181
4 00	8.0458	17	7.9833	39	9 00	8.1313	42	7.7141	194
10	8.0475	18	7.9794	41	10	8.1355	43	7.6947	209
20	8.0493	19	7.9753	43	20	8.1398	43	7.6738	226
30	8.0512	19	7.9710	45	30	8.1441	44	7.6512	245
40	8.0531	20	7.9665	48	40	8.1485	45	7.6267	267
50	8.0551		7.9617		50	8.1530		7.6000	
		21		50			46		293
5 00	8.0572	22	7.9567	53	10 00	8.1576	47	7.5707	322
10	8.0594	22	7.9514	56	10	8.1623	49	7.5385	358
20	8.0616	23	7.9458	58	20	8.1672	50	7.5027	402
30	8.0639	24	7.9400	61	30	8.1722	51	7.4625	455
40	8.0663	25	7.9339	64	40	8.1773	52	7.4170	523
50	8.0688		7.9275		50	8.1825		7.3647	
		26		67			53		612
6 00	8.0714	26	7.9208	70	11 00	8.1878	54	7.3035	734
10	8.0740	27	7.9138	74	10	8.1932	55	7.2301	911
20	8.0767	28	7.9064	78	20	8.1987	56	7.1390	1191
30	8.0795	29	7.8986	82	30	8.2043	57	7.0199	1702
40	8.0824	29	7.8904	86	40	8.2100	59	6.8497	2951
50	8.0853	30	7.8818	90	50	8.2159	60	6.5546	
7 00	8.0883		7.8728		12 00	8.2219		. . .	. . .

Arg.	Log. *A* ∓	Diff.	Log. *B* —	Diff.	Arg.	Log. *A* ∓	Diff.	Log. *B* —	Diff.
m					h m				
12 00	8.2219	+ 61	. . .	. . .	17 00	8.4737	+ 116	8.2581	+ 237
10	8.2280	62	6.5667	+3071	10	8.4853	120	8.2818	235
20	8.2342	63	6.8738	1823	20	8.4973	123	8.3053	234
30	8.2405	65	7.0561	1312	30	8.5096	126	8.3287	232
40	8.2470	66	7.1873	1032	40	8.5222	129	8.3519	231
50	8.2536		7.2905		50	8.5351		8.3750	
		67		855			133		229
13 00	8.2603	69	7.3760	734	18 00	8.5484	137	8.3979	230
10	8.2672	70	7.4494	645	10	8.5621	141	8.4209	230
20	8.2742	72	7.5139	578	20	8.5762	145	8.4439	230
30	8.2814	74	7.5717	524	30	8.5907	150	8.4669	230
40	8.2888	75	7.6241	481	40	8.6057	154	8.4899	232
50	8.2963		7.6722		50	8.6211		8.5131	
		76		447			159		233
14 00	8.3039	78	7.7169	416	19 00	8.6370	164	8.5364	236
10	8.3117	79	7.7585	392	10	8.6534	170	8.5600	238
20	8.3196	81	7.7977	371	20	8.6604	176	8.5838	240
30	8.3277	83	7.8348	353	30	8.6880	182	8.6078	244
40	8.3360	85	7.8701	336	40	8.7062	189	8.6322	248
50	8.3445		7.9037		50	8.7251		8.6570	
		87		323			196		253
15 00	8.3532	88	7.9360	310	20 00	8.7447	205	8.6823	257
10	8.3620	90	7.9670	300	10	8.7652	213	8.7080	263
20	8.3710	92	7.9970	290	20	8.7865	222	8.7343	271
30	8.3802	95	8.0260	281	30	8.8087	233	8.7614	279
40	8.3897	97	8.0541	274	40	8.8320	244	8.7893	287
50	8.3994		8.0815		50	8.8564		8.8180	
		99		267			257		297
16 00	8.4093	101	8.1082	261	21 00	8.8821	270	8.8477	308
10	8.4194	103	8.1343	256	10	8.9091	286	8.8785	322
20	8.4297	106	8.1599	251	20	8.9377	303	8.9107	336
30	8.4403	109	8.1850	247	30	8.9680	323	8.9443	354
40	8.4512	111	8.2097	244	40	9.0003	346	8.9797	375
50	8.4623	114	8.2341	240	50	9.0349	372	9.0172	398
17 00	8.4737		8.2581		22 00	9.0721		9.0570	

The factor A is *negative* when the correction is required for noon; but *positive* if it is wanted for midnight.

In like manner, treating of a planet, the factor A is *negative* if the correction is required for its superior passage, but *positive* if wanted for its inferior passage.

The factor B is *positive* when the interval is under 12 hours, and *negative* when the interval is greater.

This is what the signs affixed to the logarithms in the table indicate. In the logarithmic differences it may readily be seen that the signs merely indicate whether the logarithms are increasing or diminishing, and are entirely independent of the signs of their columns.

When the argument falls betwixt two of the times in the table, the use of the simple proportional is sufficient, and the magnitude and inequality of the differences, in some instances, need cause no uneasiness. The log. B, for example, varies unequally and rapidly near 9^h, 12^h, 14^h, and 21^h. Let us suppose, therefore, $dD = +17' 10''$, $D = 17° 2'$, these values produce a maximum in the value of dD tang. D for the sun. Notwithstanding this, the error from using the simple proportional will not be so much as $0^s,02$.

If the interval falls betwixt $11^h 50^m$ and 12^h, or betwixt 12^h and $12^h 10^m$, the second part of the equation is calculated in the first case for $11^h 50^m$, and in the second for $12^h 10^m$: the result will always be a very small quantity; with the knowledge of this, and that with an interval of 12^h the second part is $= 0$, it will be most easy to find the second part by the given interval. It may be noticed that the same precaution is required for this rare case in using Delambre's tables.

In relation to the first part of the equation, the use of the simple proportional produces only an error of $0^s,05$, with an interval of $20^h 6^m$ in the latitude of 60°, dD being $= 23' 40''$, which is the maximum for the sun. If the interval were greater, it would be a proof that the altitudes had been taken with less than two hours for a horary angle, and this might produce much greater uncertainty in the result of the observation, than any

error which could arise from the use of the simple proportional. This error, even with $21^h\ 56^m$ of interval with the same latitude, and the utmost value of d D, would amount only to $\frac{1}{4}$ of a second. But as such circumstances are very unfavourable for the determination of the time, they do not offer themselves to us in practice, and it follows that in cases which really occur, the use of the table, taking the simple proportional, is accurate to the tenth have asserted in the preceding memoir.

San Fernando, 1st May, 1820.

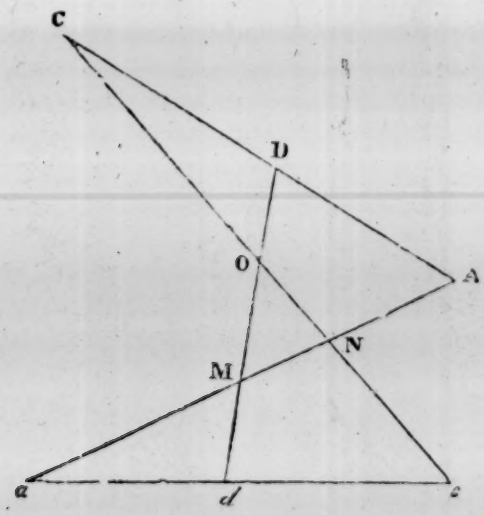
Translation finished at Liverpool,
24th December, 1821.
A. L.

ii. *An Essay on the easiest and most convenient method of Calculating the Orbit of a Comet from Observation. By* William Olbers, M D., 8vo. *Weinar.*

(Continued from Vol. XII. page 151.)

§. 58.

Supposing now *adc* to be the projection of the chord of the earth's orbit on the plane perpendicular to the middle position of the revolving radius for the earth; and *a*A, *d*D, *c*C, projections of the lines of direction on the same plane,



we have $CO : AM = \frac{CD}{\sin COD} : \frac{AD}{\sin DMA}$, and $cO : aM = \frac{cd}{\sin COD} : \frac{ad}{\sin DMA}$; consequently $CO + cO = \delta''' = \left(\frac{CD \,.\, AM}{DA} + \frac{a \,.\, M \,.\, cd}{ad}\right) \frac{\sin DMA}{\sin COD}$. Now calling $aM = f$ since $Aa = \delta'$, $AM = \delta' - f$; we have also, as in §. 38, $DMA = b'' - b'$, $COD = b''' - b''$; and the equation becomes $\delta''' = \frac{\sin.(b'' - b')}{\sin.(b''' - b'')}\left(\frac{DC}{DA}(\delta' - f) + \frac{dc}{ad} f\right)$. Again, if we put $\frac{DC}{DA} = \frac{t''}{t'} + p$, and $\frac{cd}{ad} = \frac{t''}{t'} + q$, we have, §. 57, $p = \frac{r''' \sin. \sigma}{r' \sin. \tau} - \frac{t''}{t}$, and $q = \frac{R''' \sin. (A''' - A'')}{R' \sin. (A'' - A')} - \frac{t''}{t'}$ but $\delta''' = \frac{\sin. (b'' - b')}{\sin.(b''' - b'')}\left(\frac{t''}{t'}\delta' + p\delta' - pf + qf\right)$; and since, § 38, $\frac{\sin. (b'' - b')\, t''}{\sin. (b''' - b'')\, t'} = N$, we have $\delta''' = N\left(1 + \frac{t'}{t''} p\right)\delta' + \frac{(q - p)\, f \sin. (b'' - b')}{\sin. (b''' - b'')}$.

Now we had, §. 38, $\varrho' = \frac{\delta' \cos. b'}{\sin.(A'' - \alpha')}$ and $\varrho''' = \frac{\delta''' \cos. b'''}{\sin.(A'' - \alpha''')}$, consequently $\left[\text{since } M\varrho' = \frac{N\delta' \cos. b'}{\sin. (A'' - \alpha''')}\right]$, $\varrho''' = M\left(1 + \frac{t'}{t''}\right)\varrho' + \frac{(q - p) \sin (b'' - b') \cos b''' f}{\sin (b''' - b'') \sin (A'' - \alpha''')}$; but $f = \frac{ad \sin b''}{\sin (b'' - b')} = \frac{R' \sin (A'' - A') \sin b''}{\sin (b'' - b)}$, and if we substitute this value of f in the second part of the value of ϱ''', it will become $h = \frac{R' \sin. (A'' - A')\, (q - p) \tan b''}{(\tan b''' - \tan b'') \sin (A'' - \alpha''')}$, [since $\sin (b''' - b'') = \sin b''' \cos b'' - \sin b'' \cos b'''$, which, divided by $\cos b'' \cos b'''$, gives $\tan b''' - \tan b''$]; and substituting the values of $\tan b''$ and $\tan b'''$, we have $h = \frac{R' \sin (A'' - A')\, (q - p) \tan \beta''}{\tan \beta''' \sin (A'' - \alpha'') - \tan \beta'' \sin (A'' - \alpha''')} = \frac{R' \sin (A'' - A')\, (q - p)\, m}{\tan \beta''' - m \sin (A'' - \alpha''')}$; so that the denominator is the

same that has been already employed for M, §. 38; and the whole equation becomes

$$\varrho''' = \mathrm{M}\left(1 + \frac{t'}{t''}p\right)\varrho' + \frac{\mathrm{R}' \sin(\mathrm{A}'' - \mathrm{A}')(q - p)\,m}{\tan\beta''' - m \sin(\mathrm{A}'' - \alpha''')}.$$

§ 59.

Hence we have the values of v and h in the equation $\varrho''' = (\mathrm{M}+v)\,\varrho' + h$, which determine the influence of p and q, which were before neglected, on the value of ϱ'''; and the equation might be applied to the correction of the elements already calculated. But the labour may be much abridged by observing that the ϱ' already found can differ but very little from the true value which is now to be found; and if we call the approximate value (ϱ), we shall have, since h is also small, $\frac{h\varrho'}{(\varrho)} = h$, and the equation for ϱ''' will become $\varrho''' = \mathrm{M}\left(1 + \frac{t'}{t''}p + \frac{h}{(\varrho)}\right)\varrho'$ and hence $\varrho''' = (\mathrm{M} + v)\,\varrho'$, v being $= \frac{\mathrm{M}t'}{t'}p + \frac{h}{(\varrho)}$.

§. 60.

In order, therefore, to correct the two equations for r''' and k''', we must multiply all the coefficients which contain M by $\frac{\mathrm{M} + v}{\mathrm{M}} = \mathrm{H}$, and those which contain M^2 by H^2. The equation for r' remains unaltered: and since the logarithms of the coefficients are already found, the operation is by no means difficult.

§. 61.

It will be convenient in the mean time to collect the equations for the determination of H together, for the sake of employing them more readily. When we have found (ϱ), which is the approximate value of ϱ', the time and distance at the perihelium, and ψ the true anomaly at the middle observation, as well as the differences of the anomalies τ and σ, we must compute

$$p = \frac{r''' \sin \sigma}{r' \sin \tau} - \frac{t''}{t'}$$

$$q = \frac{R''' \sin (A''' - A'')}{R' \sin (A'' - A')} - \frac{t''}{t'}, \text{ and}$$

$$h = \frac{R' \sin. (A'' - A') (q - p) m}{\text{tang } \beta''' - m \sin (A'' - \alpha''')}; \text{ and then}$$

$$H = 1 + \frac{t'}{t''} p + \frac{h}{(\varrho) M}.$$

But since in the last member of the equation for H, the h is divided by M, the expressions for these quantities having nearly the same denominators, we may take at once $\frac{h}{(\varrho) M} =$ $\frac{R' \sin (A'' - A') (q - p) mt'}{(\varrho) (m \sin (A'' - \alpha') - \text{tang } \beta') t''}$, and with this value of H we may proceed to correct the coefficients. We shall then obtain two new equations for r'' and k'' very little different from the former, from which the corrected value of ϱ' will be found so much the more easily, as its limits are so nearly ascertained by the previous determination of (ϱ). Two new hypotheses for ϱ', and an easy interpolation, will be fully sufficient for the purpose.

§. 62.

In order to illustrate the mode of computation still more completely, I shall return to the example of the comet of 1769, §. 46, 47. We have already found (§. 51) $\psi = 138°.19'.55''$, and we had $\varphi = 135°.52'.24''$, $\chi = 4°.27'.46''$, consequently $\sigma = 2°.27'.31''$, $\tau = 2°.0'.15''$; we had also $r' = 1.02367$, and $r''' = 0.83504$; consequently for p we have

Log r'	0.010160	Log r'''	9.921707
+ Log sin τ	8.543722	+ Log sin σ	8.632433
	8.553882	= L (r''' sin σ)	8.554140
		− L (r' sin τ)	8.553882
		= L. 1000.60	0.000258

And in this case $\frac{t''}{t'}$ being $= 1$, we have $p = .00060$.

For q we have $A'' - A' = 3°.53'.26''$, and $A''' - A'' = 3°.53'.49''$, hence

Log R′	0.003132	Log R‴	0.002184
+ Log sin(A′ − A′)	8.831555	+ Log sin (A‴ − A″)	8.832267
	8.834687	L (R‴ sin [A‴ − A″])	8.834451
		− L (R′ sin [A″ − A′])	8.834687
		Log .99946	9.999764

consequently $q = -.000511$. Now in order to find $\frac{h}{(\varrho)\,M}$, we have $(q - p) = -.00114$. Hence

Log R′ sin (A″ − A′)	8.834687
Log $(q - p)$ [−]	7.056905
Log m	9.648938
	5.540530
− Log .12210, the denom. §. 46	9.086716
− Log (ϱ)	9.541829
Log − 0 .00082	6.911985

We have therefore $H = 1 + p\frac{t'}{t''} + \frac{h}{(\varrho)\,M} = .99978$, and Log H = 9.999904; and in order to obtain the corrected coefficients in the equations for r''' and k'', we have only to subtract 96 from the logarithms of the terms containing M, and 192 from those which contain M^2: they will then become

$$r''' = \sqrt{(1.01011 - 1.21455\varrho' + .90829\varrho'^2)}$$
$$k'' = \sqrt{(.01868 - .10958\varrho' + .49694\varrho'^2)}$$

These equations differ so little from the former, that it is not worth while to recalculate ϱ' from them, especially as the calculation would only be a repetition of the former. It is obvious from this example, how nearly accurate the supposition, that the chords are divided in the proportion of the times, proves to be for an interval of eight days. I must however observe, that the value of M, and that of the small arcs σ, τ, $A'' - A'$, $A''' - A''$, must be computed with great care, in order that the correction deduced from them may not be erroneous.

§. 63.

It is manifest that this is a very simple method of correcting the first computation of the elements, and of obtaining the true result of the three observations in question, not very remote from each other. But the orbit of a comet can never be correctly determined from observations very near each other; partly because all observations must, for many reasons, be inaccurate, and partly for a reason not hitherto much considered, that we cannot depend on the sun's place to single seconds, and before the last labours of DELAMBRE and VON ZACH, were liable to still greater errors. An uncertainty or an error of 10″ in the sun's longitude may, in certain circumstances, lead to greater inaccuracy than an error of a minute, or even several minutes, in the observed longitude and latitude of the comet: and this fact ought to serve as a warning to computers, and induce them to determine the place of the sun at each observation with great care. But all errors in the longitude or distance of the sun, or in the observed longitude or latitude of the comet, must naturally have so much the more influence on the elements, as the observations are nearer to each other, and the portion of the orbit in question shorter.

§. 64.

Various methods have been proposed for applying the remotest observations to the correction of the approximate elements of a comet's orbit. They may, however, all be reduced to three; those of LAMBERT, of LAPLACE, and of the great NEWTON. We shall examine them all, and compare them with each other.

§. 65.

LAMBERT proposes to take the distances of the comet from the earth from the construction, or from a rough calculation, to consider their differences from the truth as differential magnitudes, the higher powers of which may be omitted in the computation; and to determine the amount of these differences from the intervals of time observed. Supposing the approximate

distances from the earth to be a, b, c, he takes for the true distances $a + x$, $b + y$, and $c + z$, expresses with these the distances of the comet from the sun, and all the chords concerned, and compares these, by means of his theorem, with the observed intervals of time: and omitting all the higher powers of x, y, and z, he, of course, obtains their values by linear equations. But this computation is not a little troublesome and tedious; and as I can assert from experience, incomparably more so than might be inferred at first sight from the examples given by LAMBERT.

§. 66.

It is far more convenient to choose two of the approximate elements and to compare them with three observations, in order to see if they agree more or less accurately with them; and then to compute the effect of small alterations in the elements on each observation. Hence the errors of these two elements will become known, and they may be corrected accordingly; and from their corrected value, the other elements of the orbit may be determined or corrected.

§. 67.

LAPLACE chooses for this purpose the time and distance of the perihelium. He then assumes three hypotheses, which, if η be the time of the perihelium, and π the distance, obtained by the approximation, may be thus represented, 1; η, π; 2; $\eta + r$, π; 3; η, $\pi + s$; and on each of these hypotheses he computes for the time of three of the remotest observations the differences of the true anomalies, and the distances of the comet from the sun. From the three distances, and the observed geocentric longitudes and latitudes, he finds again, by a calculation not very difficult, the differences of the true anomalies. If these differences agree for one of the hypotheses, the time and distance supposed in it must be correct; if not, we may obtain from these three comparisons the true time and distance, in a manner which will be explained in speaking of the NEWTONIAN method. I do not enter more particularly into the method at

present, because it has been so fully explained by LAPLACE himself in the *Memoirs of the Academy* for 1780, and after him by Pingré in his *Cométographie.*

[Note of the Editor VON ZACH. The work of LAPLACE *On the Motion of the Planets* being rare, the editor thinks it advisable to insert his formulas, in order that all the methods o· correcting the elements of the orbit may be found together and it will give room for remarking the utility of *constant logarithms* which are of use in repeating the computation upon various hypotheses. We find (1) the true anomalies $\varphi', \varphi'', \varphi'''$, from the time and distance of the perihelium, for the respective observations, by means of BARKER's table; as well as the distances from the sun, r', r'', r'''. (2) Then making $\cos \varkappa = \cos \beta \cos (A - \alpha)$ we find

Ist constant L. $= \text{Log } R + \log \sin \varkappa$
IId $= \text{Log} \sin \beta - \log \sin \varkappa$
IIId $= \text{Log } R + \log \sin (A - \alpha)$

Quantities which are obviously independent of the time and distance of the perihelium, and therefore retain the same values notwithstanding their changes. We then make (3)

$\text{Log} \sin K = \text{I c.l.} - \log r$
Angle $\Sigma = K + \varkappa$, or rather $180^\circ - K - \varkappa$
$\text{Log} \sin \lambda = \log \sin \Sigma + \text{II c.l}$
Log sin angle at the comet $= \text{III c.l.} - \log (r \cos \lambda)$
$C = \alpha \pm$ this angle $=$ hel. long. comet.

(4) Reckoning χ', χ'', and χ between the 1st and 2d, 1st and 3d, and 2d and third observations, we have

$\text{Cos } \chi' = \cos (C'' - C') \cos \lambda' \cos \lambda'' + \sin \lambda' \sin \lambda''$
$\text{Cos } \chi'' = \cos (C''' - C') \cos \lambda' \cos \lambda''' + \sin \lambda' \sin \lambda'''$
$\text{Cos } \chi''' = \cos (C''' - C'') \cos \lambda'' \cos \lambda''' + \sin \lambda'' \sin \lambda'''$; two only of these formulas being wanted, and the signs and cosines having been taken out before.

(5) Putting now $\chi' - (\varphi'' - \varphi') = \mu$, and $\chi'' - (\varphi''' - \varphi') = \nu$, we must have, if the time and distance of the perihelium are correct, $\mu = 0$, and $\nu = 0$. But as this will seldom happen, we must alter first the time of the passage of the perihelium only, and then the distance; and by comparing the three values of μ and ν thus

found, we may obtain by interpolation a hypothesis in which both will be correct; and this must again be examined by a new calculation.]

§. 68.

Notwithstanding the convenience and utility of this method' am disposed to believe that the same conciseness and facility may be attained by taking, with NEWTON, for the foundation of the three hypotheses, the longitude of the node, and the inclination of the orbit, instead of the time and distance of the perihelium, and that this method will then possess essential advantages over that of LAPLACE. I call this the NEWTONIAN method, since it could only be by want of recollection that the great EULER, who had certainly read NEWTON's works, and who had no manner of occasion to adorn himself with borrowed plumes, could have mentioned it as his own invention; "*Sequentem methodum sum assecutus.*" *Theor. met. Plan. et Com.* p. 140. It was, however, first suggested by NEWTON, and illustrated in detail by GREGORY, *Princip.* III. 42; although ascribed by many late writers to EULER, without any mention of NEWTON's name.

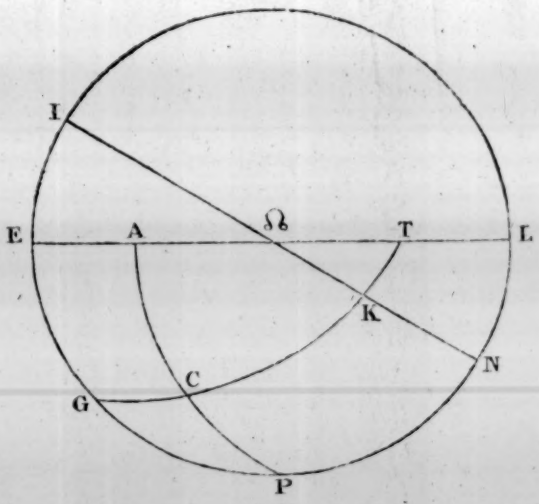
§. 69.

It has commonly been supposed that this method was only to be used with advantage, when an attempt was to be made to find the elements of an elliptic orbit; an experiment which seldom affords any satifactory result; although, if any person chooses to undertake so thankless a labour, he cannot employ a more convenient method. But it may be employed in a much shorter form for the correction of the parabolic elements: as indeed STRUYK has already employed it, although with unnecessary prolixity, and with many superfluous calculations, not being at the time acquainted with the elegant theorem of LAMBERT. *Beschr. der Staartst.* Amst. 1753. I adopted it in a shorter form 17 years ago, in determining the elements of the comet of 1779, from observations which I had made almost without any instruments. *Astr. Jahrb.* Berl. 1782.

§. 70.

In this method we have occasion for the solution of the problem, to determine, from the given position of the orbit with respect to the ecliptic, and the geocentric longitude and latitude of the comet, the heliocentric distance from the node, and the distance of the comet from the sun. NEWTON takes this solution for granted: GREGORY, EULER, and STRUYK have entered into the particulars of it; and LEXELL, in a separate essay; and Professor NORDMARK in an occasional Programma, have endeavoured to simplify and accommodate to practice the formulas subservient to it. And yet it seems to be possible to obtain a more convenient practical solution than has hitherto been made public. It has been usual to employ for the purpose plane trigonometry only; but the problem evidently belongs to sphetical trigonometry, since it depends on the relative situation of two planes, the first determined by the centres of the sun, the earth, and the comet; the second, or the orbit of the comet, by the nodes and inclination appropriate to it.

§. 71.



Now let E A ☊ T L be the ecliptic, ☊ the node, here the descending node, I ☊ N the orbit as seen from the sun, T the earth's place, and C the observed geocentric place of the comet. The great circle T K C G being drawn through the place of the comet, its heliocentric place will be K, ☊

K the heliocentric distance of the comet from the node, T K its heliocentric distance from the earth, K C the angle at the comet, and lastly the supplement of T C the geocentric distance of the comet from the sun. [In order that C may represent the geocentric place of the comet, it must be that point of the sphere which is indicated by a line drawn from the sun, supposed to be in its centre, parallel to the direction of the comet from the earth: and this line will make an angle with the revolving radius of the comet, equal to the alternate angle at the comet, which will therefore be represented by K C. Tr.] Now it is obvious that all these distances may be found by the solution of two spherical triangles.

(1) In the right-angled triangle A C T we have T A, the difference of the earth's longitude and the geocentric longitude of the comet, and A C the observed latitude of the comet: whence we have (i.) $\cos TC = \cos TA \cos AC$; and (ii.) $\cot ATC = \cot AC \sin TA$.

(2) In the oblique angled triangle ☊ K T, we have ☊ T the difference of the longitude of the node and of the earth, the angle T ☊ K the inclination of the orbit, and the angle ☊ T K = A T C; hence we find ☊ K and T K by the formulas (iii.) $\tan \frac{1}{2} (☊K + TK) = \frac{\cos \frac{1}{2} (☊TK - T☊K)}{\cos \frac{1}{2} (☊TK + T☊K)} \tan \frac{1}{2} ☊T$,

(iv.) $\tan \frac{1}{2} (☊K - TK) = \frac{\sin \frac{1}{2} (☊TK - T☊K)}{\sin \frac{1}{2} (☊TK + T☊K)} \tan \frac{1}{2} ☊T$.

We have then $KC = TC - TK$, and R being the distance of the earth from the sun, and r that of the comet, we have (v.) $r. = \frac{R \sin TC}{\sin KC}$.

§. 72.

If we compare these formulas with those which have been hitherto employed, we shall be aware of their great convenience especially in the correction of the elements of a comet's orbit. Euler, for example, employs in his *Recherches sur la vraie orbite elliptique de la comète de* 1769, eight proportions, instead of the five here laid down. The whole eight he is obliged to compute for each of the hypotheses, which he assumes for

the longitude of the node and the inclination of the orbit; but here the first, second, and the numerator of the fraction remain the same for all three hypotheses; and besides the coefficient of tang $\frac{1}{2}$ ☊ T is the same for two hypotheses. In short EULER requires 75 logarithms for each observation, this method only 43; while in the computations of LEXELL and NORDMARK about 57 or 60 are employed.

§. 73.

The problem having been reduced to the solution of two cases of spherical triangles, we might easily introduce differential expressions, instead of the three hypotheses, or we might compute the effect of small alterations in the longitude of the node and the inclination of the orbit on the values of ☊ K and r. But I have found by experience that the advantage of this mode of proceeding is not considerable: the quantities themselves may as easily be computed on three hypotheses as the differential formulas, which I am the less disposed to insert here, as they may be investigated with very little trouble.

§. 74.

Having then assumed three hypotheses for the longitude of the node, and for the inclination, we compute upon each of them for the three observations ☊ K $= \xi$, and r: and then the chords between the first and second, and the first and third observations from the formula

$$k' = \sqrt{([r'' - r']^2 + 4\, r'\, r'' \sin \tfrac{1}{2} (\xi'' - \xi')^2)}$$
$$k'' = \sqrt{([r''' - r']^2 + 4\, r'\, r''' \sin \tfrac{1}{2} (\xi''' - \xi')^2)}$$

We then find from k', k'', and r', r'', r''', the corresponding times between the first and second, and the first and third observations. By comparing these times with those which have been observed, we obtain the true longitude of the node, and the true inclination of the orbit; and hence, by an easy interpolation, the true values of r', r''', ξ', and ξ''', by means of which we find the remaining elements.

§. 75.

In order to have a clear view of the whole proceeding, we may represent the three hypotheses in this manner:

	1 Hyp.	2 Hyp.	3 Hyp.
Longitude of the ☊	☊	☊ $+ p$	☊
Inclination	i	i	$i + q$

p and q amounting to 10, 15, 20, or even more minutes. For each of these hypotheses, and for three observations, we calculate, according to §. 71, ξ', ξ'', ξ''', r', r'', and r'''; and then, by §. 74, k' and k''. We next find the times in each hypothesis, between the first and second, and between the first and third observations, calling them, 1st τ' and τ'', 2ndly $\tau'' + l$, $\tau'' + o$, 3rdly $\tau' + m$, $\tau'' + s$; while the observed intervals are t' and t''. If now the true longitude of the node be ☊ $+ x$, and the true inclination of the orbit $i + y$, we have the equations

$$\frac{xl}{p} + \frac{ym}{q} = t' - \tau', \quad \frac{xo}{p} + \frac{ys}{q} = t'' - \tau''; \text{ and hence } x =$$

$$\frac{(t' - \tau')\, sp - (t'' - \tau'')\, mp}{mo - sl}, \text{ and } y = \frac{(t' - \tau')\, oq - (t'' - \tau'')\, lq}{mo - sl};$$

giving us the true longitude of the node, and the true inclination of the orbit. The true values of r'', r''', ξ', and ξ''', are then found by interpolation; taking for any magnitude which may have been found in the three hypotheses B, B $+ f$, and B $+ g$, the true value B $+ \frac{fx}{p} + \frac{gy}{q}$. It is obvious that, in order to obtain all possible accuracy, we must renew the operation with three new hypotheses for the longitude of the node and the inclination, less remote from each other, if x and y should be found materially greater than p and q, or if large values of p and q had been employed, such as 50, 60, or more minutes. For the method is only so far correct, as the alterations of all the other magnitudes concerned may be considered as proportional to those of the longitude of the node, and of the inclination of the orbit, which is only admissible for small values of p and q; a limitation which is equally applicable to the method of Laplace, and to that which remains to be explained.

[Note of the Editor. The formulas used by LAPLACE considerably resemble these. If the μ and ν, of §. 67, be made for the three hypotheses μ', μ'', μ''', and ν', ν'', ν''', we have

$$y(\mu' - \mu'') + x(\mu' - \mu''') = \mu', \text{ and}$$

$$y(\nu' - \nu'') + x(\nu' - \nu''') = \nu';$$ the resolution and use of these equations being perfectly similar to those of §. 75, and y being the factor by which the alteration of the perihelium distance is multiplied, and x the factor of the alteration of the time of the passage through the perihelium, by means of which the true alterations of these elements are determined. Sometimes, however, it may become necessary to take the second differences into consideration, and the editor has employed LAPLACE's formulas with advantage in the calculations of several comets: he therefore inserts them as applied to LAPLACE's own method; but they may be accommodated, without difficulty, to every other. We are to compute the values of μ and ν for the five following hypotheses; (1) with the elements found by the first approximation; (2) with a small alteration of the perihelium distance; (3) with twice as great an alteration; (4) with the approximate distance unaltered, and with a small alteration of the time; (5) with twice as great an alteration: now if we distinguish the five values of μ and ν by the number of accents, and make x and y the factors of the alterations in the 2d and 4th hypotheses, which are required to find the true alterations of the elements, we may find the values of x and y by the following equations. $0 = (4\mu'' - 3\mu' - \mu''')y + (\mu''' - 2\mu'' + \mu')y^2 + (4\mu'''' - 3\mu' - \mu''''')x + (\mu''''' - 2\mu'''' + \mu')x^2 + 2\mu'$, and $0 = (4\nu'' - 3\nu' - \nu''')y + (\nu''' - 2\nu'' + \nu')y^2 + (4\nu'''' - 3\nu' - \nu''''')x + (\nu''''' - 2\nu'''' + \nu')x^2 + 2\nu'$. We may also observe that we may resolve these equations by extermination, but that we are led by a troublesome operation to an equation of the fourth degree; and that it is therefore more convenient to find approximate values with the omission of the quadratic terms x and y^2, and then to compute these terms with the values so found and to renew the operation, so that x and y may be deduced from the linear equations thus obtained.]

§. 76.

Besides these two methods of correction, I shall explain another which really seems to be the most commodious, where the elements of a parabolic orbit only are in question: and even if it were less advantageous, it would be of use to be in possession of a variety; especially as the two former methods sometimes fail. LAPLACE's method is awkward when the angle at the comet in one of the three observations is very nearly a right one: and NEWTON's computation cannot be employed, when either the inclination of the orbit is very small, or the earth is very near the line of the nodes in one of the observations. Instead of the hypotheses respecting the time and distance of the perihelium, or the situation of the plane of the orbit, we may make three suppositions respecting the curtate distances of the comet from the sun, in two of the remotest observations in our possession; we may compute these curtate distances from the approximate orbit, calling then Δ' and Δ''' and then assume

	1 Hyp.	2 Hyp.	3 Hyp.
1 Obs.	Δ'	$\Delta' + m$	Δ'
3 Obs.	Δ'''	Δ'''	$\Delta''' + n$

We then compute, for Δ' and $\Delta + m$ and the observed position of the comet, the heliocentric longitude and latitude at the time of the first observation, and for Δ''' and $\Delta''' + n$ the heliocentric longitude and latitude in the third observation. These computations are very easy: for the angle at the projected place of the comet, in the plane of the ecliptic, is found by the equation $\sin c = \frac{R \sin (A - \alpha)}{\Delta}$; observing, however, to take the angle acute or obtuse, according to circumstances; and hence we find ε, the elongation of the comet from the earth $= 180^\circ - c - (A - \alpha)$; and for the heliocentric latitude, $\tang \lambda = \frac{\tang \beta \sin (A - \alpha)}{\sin \varepsilon}$: we then compute, according to §. 42, the longitude of the ascending node, and the inclination of the orbit, for each of the three hypotheses; and since $r' = \Delta' \sec \lambda'$ and $r''' = \Delta''' \sec \lambda'''$, we hence find the true anomalies in both ob-

servations, the distance of the perihelium, and the time from the perihelium to the first and third observations; which gives us the time that ought to elapse between these observations, according to each of the hypotheses: and hence we have the first comparison. We then add, for each of the three orbits, to the time between the perihelium and the first observation, the observed time from the first to a second observation, sufficiently remote from both the others, and compute the respective geocentric longitude; or the latitude, if the latitudes vary more rapidly than the longitudes: and the place so computed gives, together with the observation, the second comparison.

§. 77.

The whole proceeding will therefore stand thus,

		1 Hyp.	2 Hyp.	3 Hyp.	True Orbit.
Curtate dist.	1 Obs.	Δ'	$\Delta'+m$	Δ'	$\Delta'+x$
	3 Obs.	Δ'''	Δ'''	$\Delta'''+n$	$\Delta'''+y$
Time between 1 and 3 obs.		τ	$\tau+p$	$\tau+q$	t'' obs.
Longitude in 2 obs.		α	$\alpha+\nu$	$\alpha+s$	α'' obs.

We have then $\frac{px}{m} + \frac{qy}{n} = t'' - \tau$, and $\frac{rx}{m} + \frac{sy}{n} = \alpha'' - \alpha$; whence we obtain the value of x and y in the same manner as in §. 75. If now m and n have been assumed not too great, and x and y are still smaller, or not considerably larger, the whole of the elements of the orbit may be readily found by interpolation.

§. 78.

It must, however, be remembered, that three complete observations are more than sufficient to determine the orbit of a comet considered as a parabola: that is, if the orbit were not correctly parabolic, or if the observations were erroneous, we could only represent by a parabola three longitudes and two latitudes, or two longitudes and three latitudes: and for this reason I have employed, in the method of correction here explained, only the longitude or the latitude of the middle observation; while in the methods of Lambert and Laplace, as well as in that of Newton here applied to the parabola, we appear to satisfy three complete observations. But this appear-

ance is fallacious; for if there is any want of regularity in the assumed grounds of calculation, some condition of the problem must remain unfulfilled, if we imagine that we have represented the whole of the observations. Thus in LAMBERT's method, §. 65, we shall have determined the intervals of time correctly, but the points found for the places of the comet will not lie in a plane passing through the sun. According to LAPLACE, §. 67, the time and distance of the perihelium, and the true anomalies derived from them, will agree with the observations; but the heliocentric places, derived from them, will again deviate from a great circle. The NEWTONIAN determination of the longitude of the node, and the inclination of the orbit, giving r', r'', r''', and k', k'', correctly, and thence the time agreeing with the observed interval, will indicate places not found in one and the same parabola. In all these cases, therefore, we shall find not one parabola, but strictly speaking three, differing more or less from each other according to the accuracy of the observations, or to the extent of the actual deviation of the comet from a parabola. In LAPLACE's method, the time and distance of the perihelium only are the same for all three; in NEWTON's, the plane of the orbit only is the same; in LAMBERT's, all the five elements belong to three different parabolas. We must adhere in practice to that orbit which agrees perfectly with the first and third observations; passing through the extreme points which have been determined.

§. 79.

The condition that all the points of the orbit must lie in a plane, passing through the sun's centre, is in itself the most essential part of the theory: and hence the NEWTONIAN mode of correction has the advantage, since it satisfies this principal condition: it has also the superior convenience of being immediately applicable to the determination of the elements of an elliptic orbit, if it should happen that a parabola is not sufficient for the observations.

§. 80.

In order to determine if this is the case, we must compute

again ξ'' and r'' from the parabolic elements which have been obtained for the two remotest observations, and compare these values with those which have been already determined according to §. 75. If there is any material difference in the values, supposing that p and q have not been assumed too great, that the observations are accurate, and that they are sufficiently remote, we may then attempt to find an elliptic orbit which shall agree better than the parabolic. I have not been able in this instance to abridge materially the method of EULER, which he has given in the two works already quoted. Instead of the chords k', k'', as soon as we have found ξ', ξ'', ξ''', r', r'', and r''', we must immediately determine the parameter of the ellipse for each of the three hypotheses, by the formula

$$b = \frac{\sin(\xi'' - \xi') + \sin(\xi''' - \xi'') - \sin(\xi''' - \xi')}{\frac{\sin(\xi'' - \xi')}{r'''} + \frac{\sin(\xi''' - \xi'')}{r'} - \frac{\sin(\xi''' - \xi')}{r''}},$$

which is much more convenient than that which EULER has given in his *Theoria*, but is essentially the same with the calculation contained in his *Recherches*. From the parameter thus found we easily obtain the true anomaly for the first observation, the perihelium distance, the times from the perihelium, and the interval between the observations. For this purpose I prefer the formulas of the *Theoria* to those of the *Recherches*. By comparing the computed with the observed intervals, we determine, as for the parabola, the correction of the longitude of the node and of the inclination, and we thus obtain the true value of the elliptic elements by interpolation.

[Note 6. The elegant theorem here introduced may be thus demonstrated. Calling the distances a, b, and c, the angles formed with the greater axis x, $x + y$, and $x + z$, the parameter p, and the eccentricity e, the greater axis being 1, we obtain, from the well known property of the ellipsis, $a = \frac{p}{1 + e \cos x}$ $b = \frac{p}{1 + e \cos(x + y)}$, and $c = \frac{p}{1 + e \cos(x + z)}$, whence $\cos x = \frac{p-a}{ae}$ $\cos(x + y) = \frac{p-b}{be}$, and $\cos(x+z) = \frac{p-c}{ce}$; but $\cos(x + y) = \cos x \cos y - \sin x \sin y$, whence $\sin y =$

$\frac{\cos x \cos y - \cos(x+y)}{\sin x} = \frac{p-a}{ae \sin x} \cos y - \frac{p-b}{be \sin x}$; and in the same manner $\sin z = \frac{p-a}{ae \sin x} \cos z - \frac{p-c}{ce \sin x}$. Now $\sin(z-y) = \sin z \cos y - \sin y \cos z = \frac{p-a}{ae \sin x} \cos y \cos z - \frac{p-c}{ce \sin x} \cos y - \frac{p-a}{ae \sin x} \cos y \cos z + \frac{p-b}{be \sin x} \cos z = \frac{p-b}{be \sin x} \cos z - \frac{p-c}{ce \sin x} \cos y$; consequently $\frac{p-a}{a} \sin(z-y) = \frac{p-a}{a} \cdot \frac{p-b}{bc \sin x} \cos z - \frac{p-a}{a} \cdot \frac{p-c}{ce \sin x} \cos y = \frac{p-b}{b} \sin z - \frac{p-c}{c}$ sin and $\frac{p}{a} \sin(z-y) - \frac{p}{b} \sin z + \frac{p}{c} \sin y = \sin(z-y) - \sin z + \sin y$; whence $p = \frac{\sin y + \sin(z-y) - \sin z}{\frac{\sin y}{c} + \frac{\sin(z-y)}{a} - \frac{\sin z}{b}}$, which is Dr. Olbers's formula. Tr.]

§. 81.

It will seldom or never occur to us to compute the elliptic orbit for the sake of any material utility or advantage. The portion of the orbit, which is in the neighbourhood of the sun, may almost always be so accurately determined by the parabolic hypothesis, that we can represent and predict and judge of its course, and its distance from the earth and the sun, and identify it upon a second appearance with sufficient precision. And this appears to me to be the whole object of the calculation of a comet, since the determination of an elliptic orbit can never ascertain the period of revolution with any certainty, the deviations from the parabolic orbit being always complicated with the errors of observation: and these errors are certainly in many cases greater than could have been imagined, partly from the nature of the light and from the form of the comet, and partly from the imperfections of our catalogues of fixed stars. The comet of 1770, indeed, seems to afford a remarkable and well known exception to the universality of this observation. But without wishing to decide upon this point, we may at least remark, first, that the

observations before the perihelium may have been the more erroneous, as the comet, though without a tail, had still a very considerable apparent diameter, and it is not easy to distinguish the centre of gravity of this mass of vapours as the proper point of observation: and secondly, that the method of NEWTON or of EULER, by which LEXELL determined the ellipsis and the period of this comet, was particularly ill adapted to the occasion, as the orbit was so little inclined to the ecliptic. I do not, however, deny that this paradoxical comet described an ellipsis considerably different from a parabola, since observations so rough as those of LAMBERT, (*Beyträge* III. 318,) are sufficient to show the inadequacy of a parabolic orbit, and even the observations after the perihelium, taken separately, cannot be represented by any one parabola. It is remarkable, that so great a mathematician as DUSE'JOUR should have fancied, tha the had discovered more than one parabola which was capable of giving the observed places at three different times with perfect accuracy. (*Traité* II. xv).

§. 82.

For the determination of elliptic elements the greatest care and accuracy is required in the choice and management of the observations: and we must pay proper regard to parallax, aberration, and nutation. It might, perhaps, be eligible to compute all the observations with the greatest accuracy for a parabola approaching very near to the orbit: the differences of the observations from the computation, so far as they depend on the elliptic form of the orbit, must then exhibit a uniform and regular increase and decrease. Abrupt changes and irregularities imply errors either in the observation or in the computation; and we must not omit to pay attention to single seconds. With these precautions we may be able to form some estimate of the probable errors of the observations, and to make allowance for them; we may then proceed to the investigation of an elliptic orbit, especially when the comet has been seen in both branches of the curve, both before and after its passage through the perihelium.

[The Table of Comets in a future Number.]

Art. XIII.—*Results of some Astronomical Observations made in Blackman-Street, during the Months of March, April, and May*, 1822, *by* James South, F.R.S., *in Lat.* 51° 30′ 3″ N., *Longitude* 21″.76 W.

Eclipses of Jupiter's Satellites.

(Mean Time.)

March 1,	Emersion of 1st Sat. at	h. ′ ″ 6 57 36.60	with 5 feet Equatorial
	.....	6 57 43.60	with 30 inch Gregorian.

Diameter of Venus taken in the direction of her Cusps, at about two o'clock, P.M.

March 3,	The planet distant from her inferior conjunction	6 days	mean of	10 measures = 59.936″

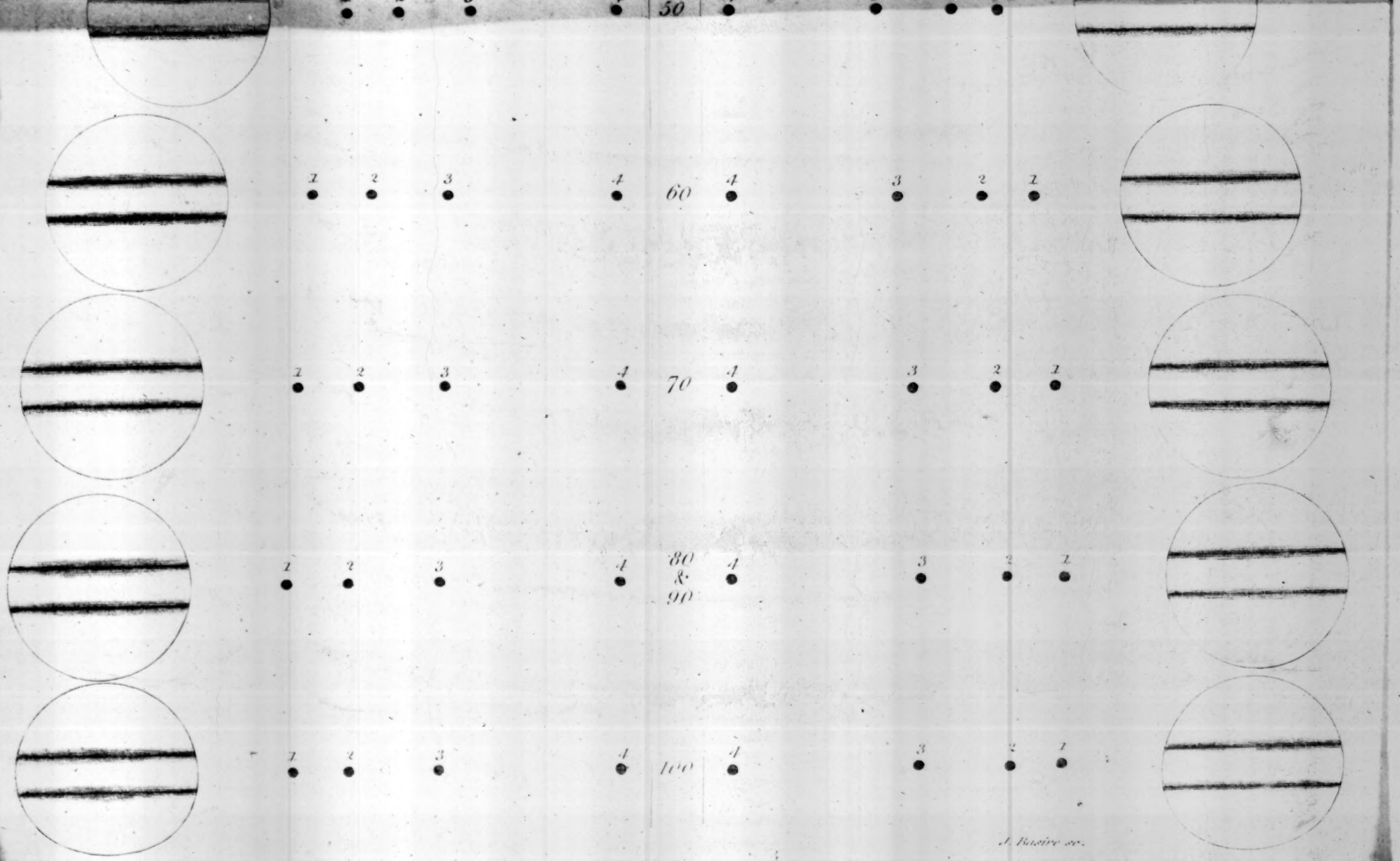
Occultations of Fixed Stars by the Moon's dark Limb.

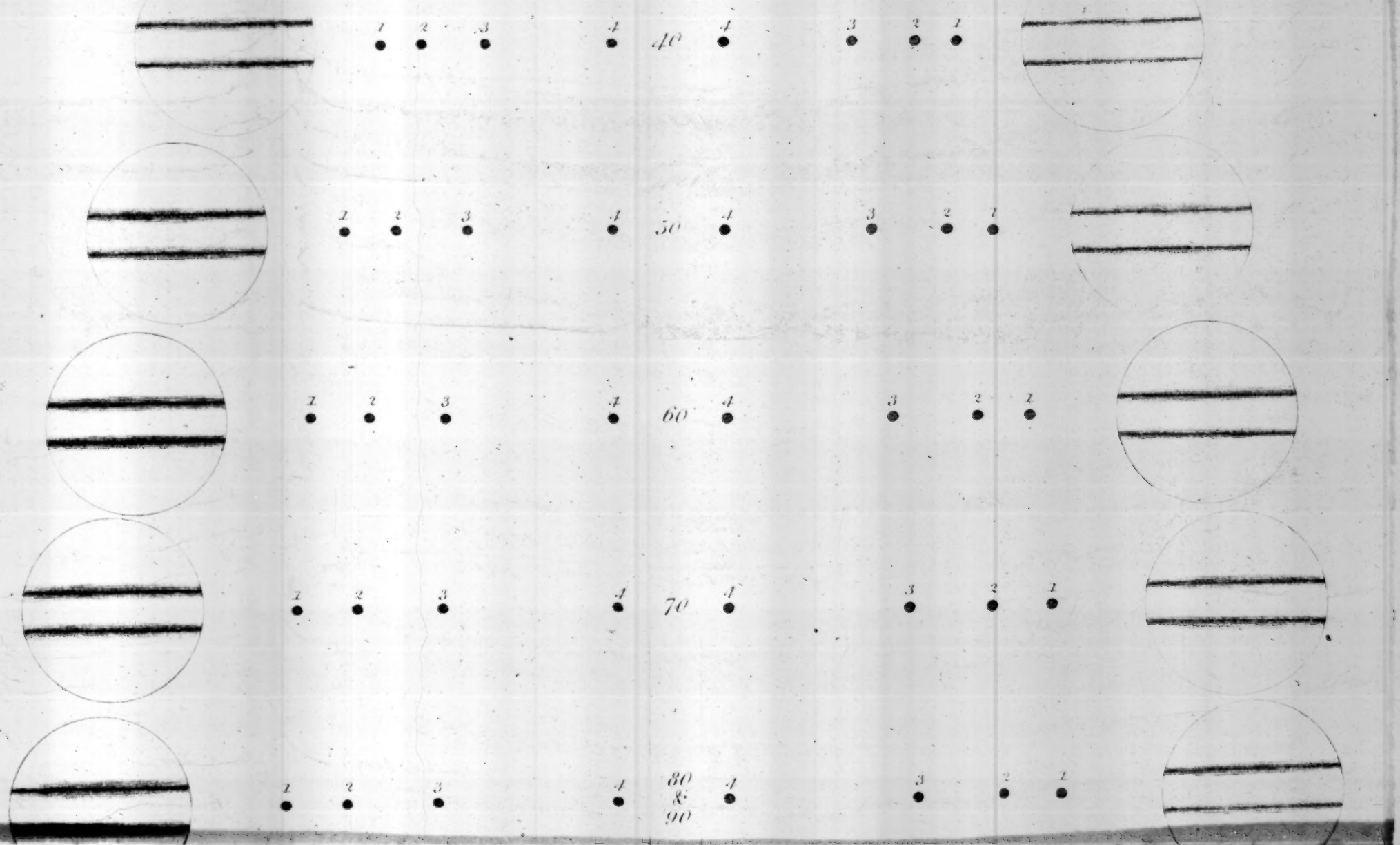
Star's AR.	Star's Declin.	Mag.	Time of immersion (in Sidereal time).	Instrument employed.
March 1.			H. ′ ″	
5 44 30 ±	27 41 ± N	6	5 57 4.75	5 feet Equatorial.
5 45 57 ±	27 17 ± N	6	6 53 29.67	Do.
5 47 58 ±	27 32 ± N	6	7 38 35.79	Do.
5 49 10 ±	27 15 ± N	5	8 18 9.71	Do.
5 49 56 ±	27 33 ± N	5	8 39 14.82	Do.
March 2.				
6 47 40 ±	26 19 ± N	6.5	6 21 9.32	Do.
.....	.....	.....	6 21 8.92	9 feet Equatorial.
6 48 22 ±	26 9 ± N	5	6 50 55.83	Do.
.....	.....	.....	6 50 55.73	5 feet Equatorial.
May 24.				
5 57 20 ±	22 6 ±	6.5	13 31 45.98	Achromatic of 3¼ aperture and 45 inches focus.
May 26.				
9 49 9 ±	11 48 ±	7	15 31 20.39	
May 28.				
11 20 16 ±	0 38 ±	8	14 36 52.49	5 feet Equatorial.

No tremulous appearance of either of these Stars, no projection of either of them upon the moon's disk—the disappearance of all instantaneous.

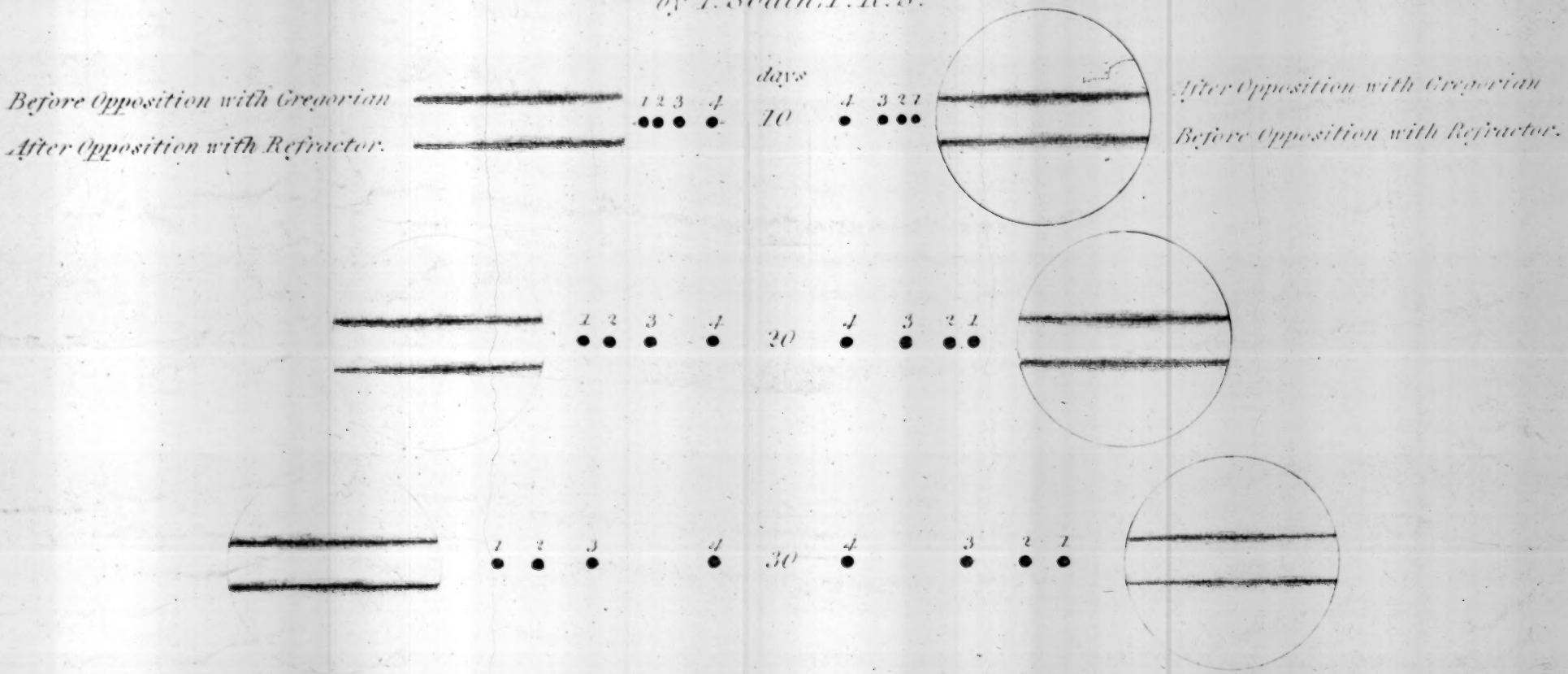
An object glass, 3¼ inches in diameter, and of 45 inches focal length, having been made for me by Tulley, from formulæ given by Mr. Herschel, in a paper recently published in the *Philosophical Transactions*, it became a matter of much interest to ascertain what merit it might possess; and for this purpose it was directed to some of those double stars in which proximity or faintness renders one of them difficult to be seen, and the accompanying diagrams, plate V., are faithful representations of the results: what, therefore, Mr. Herschel's *theory* told him would be good, Mr. Tulley's *practice* has declared so. The magnifying power employed upon the stars was about 300—upon the Nebula about 50.

At the request of several astronomical friends, the corrections in right ascension of the thirty-six principal stars are here resumed; and tables for converting space into time and the contrary, are subjoined for the convenience of the practical astronomer.


50
1 2 3 4 60 4 3 2 1
1 2 3 4 70 4 3 2 1
1 2 3 4 80 & 90 4 3 2 1
1 2 3 4 100 4 3 2 1
J. Basire sc.


1 2 3 4 40 4 3 2 1
1 2 3 4 50 4 3 2 1
1 2 3 4 60 4 3 2 1
1 2 3 4 70 4 3 2 1
1 2 3 4 80 & 90 4 3 2 1

A Table shewing the Distances
From the Limb at which the Satellites of Jupiter
immerge into & emerge from his Shadow during 100 days
from the Planets Opposition.
by J. South, F.R.S.



[illegible]

ART. XIV.—THE MEAN PLACES OF 46 GREENWICH STARS, Reduced to Jan. 1, 1822.

NAMES OF STARS.	Right Ascension.	N. P. D.	Declination.
γ Pegasi	0 4 5.00	75 48 19.75	14 11 40.25 N
α Cassiopeiæ .	0 30 27.61	34 26 24.48	55 33 35.52 N
Polaris . . .	0 57 30.03	1 38 26.67	88 21 33.33 N
α Arietis	1 57 9.71	67 22 59.89	22 37 0.11 N
α Ceti	2 52 59.13	86 36 48.69	3 23 11.31 N
α Persei	3 11 39.65	40 46 50.87	49 13 9.13 N
Aldebaran . .	4 25 43.14	73 51 23.77	16 8 36.23 N
Capella . . .	5 3 33.41	44 11 39.35	45 48 20.65 N
Rigel	5 5 59.32	98 24 50.01	8 24 50.01 S
β Tauri	5 15 2.97	61 33 9.23	28 26 50.77 N
α Orionis . . .	5 45 32.42	82 38 3.30	7 21 56.70 N
Sirius	6 37 18.12	106 28 39.39	16 28 39.39 S
Castor	7 23 13.81	57 43 50.43	32 16 9.57 N
Procyon . . .	7 29 58.97	84 19 32.77	5 40 27.23 N
Pollux	7 34 24.85	61 33 8.45	28 26 51.55 N
α Hydræ . . .	9 18 50.51	97 53 27.57	7 53 27.57 S
Regulus . . .	9 58 53.16	77 9 58.28	12 50 1.72 N
α Ursæ Maj. . .	10 52 39.65	27 17 25.20	62 42 34.80 N
β Leonis	11 39 58.59	74 25 58.01	15 34 1.99 N
β Virginis . . .	11 41 25.59	87 13 54.66	2 46 5.34 N
γ Ursæ Maj. . .	11 44 25 53	35 18 55.25	54 41 4.75 N
Polaris, S. P.	12 57 30.03	1 38 26.67	88 21 33.33 N
Spica Virg. .	13 15 49.74	100 13 41.18	10 13 41.18 S
η Ursæ Maj. . .	13 40 31.19	39 47 41.61	50 12 18.39 N
Arcturus . .	14 7 32.90	69 53 9.87	20 6 50.13 N
1 α Libræ . . .	14 40 51.61	105 14 56.00	15 14 56.00 S
2 α Libræ . . .	14 41 3.05	105 17 40.13	15 17 40.13 S
β Ursæ Minor .	14 51 19.72	15 7 1.23	74 52 58.77 N
α Coronæ Bor. .	15 27 9.40	62 40 47.76	27 19 12.24 N
α Serpentis . .	15 35 30.55	83 0 24.79	6 59 35.21 N
Antares . . .	16 18 30.55	116 1 33.34	26 1 33.34 S
α Herculis . . .	17 6 32.29	75 23 54.55	14 36 5.45 N
α Ophiuchi . .	17 26 40.70	77 18 6.75	12 41 53.25 N
γ Draconis . .	17 52 28.65	38 29 9.85	51 30 50.15 N
α Lyræ	18 30 54.95	51 22 33.45	38 37 26.55 N
γ Aquilæ . .	19 37 47.98	79 48 45.46	10 11 14.54 N
α Aquilæ . .	19 42 6.00	81 35 37.27	8 24 22.73 N
β Aquilæ . .	19 46 34.31	84 1 46.99	5 58 13.01 N
1 α Capricorni	20 7 46.56	103 2 59.00	13 2 59.00 S
2 α Capricorni	20 8 10.31	103 5 15.75	13 5 15.75 S
α Cygni	20 35 22.14	45 21 3.33	44 38 56.67 N
α Cephei	21 14 19.55	28 9 58.45	61 50 1.55 N
β Cephei	21 26 19.58	20 13 9.24	69 46 50.76 N
α Aquarii . . .	21 56 38.44	91 10 45.81	1 10 45.81 S
Fomalhaut. .	22 47 47.64	120 33 47.60	30 33 47.60 S
α Pegasi . . .	22 55 54.20	75 44 57.10	14 15 2.90 N
α Andromedæ .	23 59 12.52	61 53 30.33	28 6 29.67 N

JULY	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran	Capella.	Rigel.	β Tauri.	ι Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Vir.	Arcturus.
	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″
1	+2.35	+1.82	+1.55	+1.33	+1.41	+0.93	+1.34	+1.11	+0.57	+1.43	+1.07	+1.40	+1.14	+1.64	+2.09	+2.04	+2.50	+2.76
2	39	85	58	35	44	95	37	13	59	44	08	40	13	64	08	04	49	75
3	42	88	61	38	47	97	39	15	60	45	09	41	13	63	07	03	48	74
4	45	91	63	40	50	99	41	16	61	45	09	42	13	63	07	02	47	73
5	48	95	66	42	53	+1.01	43	18	62	46	10	43	13	63	06	01	46	71
6	51	98	69	45	56	03	45	20	63	47	11	44	13	62	05	00	45	70
7	55	+2.01	72	47	59	05	48	22	65	48	12	44	12	62	04	00	44	69
8	58	05	75	50	62	07	50	23	66	49	12	45	12	61	03	+1.99	43	68
9	61	08	78	52	65	09	52	25	67	50	13	46	12	61	02	98	42	67
10	64	11	81	55	68	11	55	27	69	51	14	47	12	61	01	97	41	66
11	67	15	84	58	71	13	57	29	71	53	15	48	12	60	00	96	41	65
12	70	18	87	61	74	16	60	31	73	54	17	50	12	60	00	96	39	64
13	73	22	90	64	78	19	64	34	75	57	19	52	13	60	00	96	39	63
14	76	26	93	67	81	21	67	36	76	58	20	53	13	60	00	95	38	62
15	79	30	96	70	84	23	70	38	77	60	21	54	13	60	+1.99	95	37	61
16	82	33	99	73	87	25	72	40	79	61	22	55	13	60	98	94	36	60
17	85	36	+2.02	75	90	28	75	42	80	63	24	57	13	60	97	94	35	58
18	88	40	05	78	94	30	77	44	82	64	25	58	13	59	96	93	34	57
19	91	43	08	81	97	32	80	46	83	66	26	59	13	59	95	92	33	56
20	94	46	11	84	+2.00	34	83	48	85	68	27	61	13	59	94	91	32	55
21	97	49	14	87	04	37	86	51	86	69	28	62	14	59	93	91	31	53
22	99	53	17	90	07	39	88	53	88	71	30	64	14	59	93	90	30	52
23	+3.02	56	21	93	11	42	91	55	90	73	31	65	14	59	92	89	29	50
24	05	59	24	95	14	44	94	57	91	74	32	67	14	59	91	89	28	49
25	08	62	27	98	18	46	97	60	93	76	34	69	15	60	90	88	26	48
26	11	65	30	+2.01	21	49	+2.00	62	95	78	35	70	15	60	89	87	25	46
27	13	69	34	04	25	51	02	64	97	80	36	72	15	60	89	86	24	45
28	16	72	37	07	28	54	05	67	98	81	38	73	16	60	88	86	23	43
29	19	76	40	10	32	56	08	69	+1.00	83	39	75	17	60	87	85	22	42
30	22	80	43	13	36	59	11	71	02	85	41	77	18	60	86	85	21	41
31	25	84	46	16	40	62	15	74	04	87	43	79	18	61	86	84	20	39

JULY continued	α Libræ.	α Cor.Bor.	α Serpent.	Antares.	α Herculis	α Ophiuch	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	α Capric.	α Cygni.	α Aquarii.	Fomalh.	α Pegasi.	α Androm.	Polaris.
	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	H. M. S.
1	+ 3.07	+ 3.06	+ 3.17	+ 3.95	+ 3.33	+ 3.38	+ 3.16	+ 3.38	+ 3.43	+ 3.44	+ 3.79	+ 2.86	+ 3.14	+ 3.44	+ 2.68	+ 2.27	0 . 57 . 22.50
2	07	06	17	95	33	38	16	39	44	45	81	88	16	47	71	30	23.32
3	06	05	16	95	33	38	17	40	46	47	83	89	19	51	74	34	24.10
4	05	04	16	95	33	38	17	41	47	48	84	91	21	54	76	37	24.84
5	04	03	15	94	33	39	18	43	48	50	86	93	24	57	79	40	25.54
6	03	02	15	94	33	39	18	44	50	51	88	95	26	60	82	44	26.22
7	03	02	14	94	33	39	18	45	51	53	90	97	29	63	85	47	26.90
8	02	01	14	93	33	39	19	47	53	54	91	99	31	67	88	51	27.59
9	01	00	13	93	33	40	19	48	54	56	93	+ 3.01	34	70	91	54	28.31
10	00	+ 2.99	12	93	33	40	19	49	55	57	94	02	36	73	94	57	29.06
11	+ 2.99	98	12	92	34	40	19	50	56	58	96	04	38	77	96	60	29.86
12	99	97	11	92	34	40	19	51	57	59	98	06	41	80	99	63	30.69
13	98	96	11	92	34	41	20	53	59	61	+ 4.00	08	44	84	+ 3.02	67	31.51
14	98	95	10	92	34	41	20	55	60	62	01	09	47	87	05	71	32.37
15	97	94	10	91	33	41	20	56	62	64	02	10	49	91	08	75	33.18
16	96	93	09	91	33	41	20	57	63	65	04	12	51	94	10	78	33.96
17	95	91	09	90	33	40	20	58	64	66	05	13	53	97	13	81	34.69
18	94	90	08	90	32	40	20	59	65	67	07	15	56	+ 4.00	15	84	35.39
19	93	89	07	89	32	40	20	60	66	68	08	16	58	03	18	87	36.05
20	92	88	06	88	31	40	19	60	67	69	09	17	60	06	20	90	36.69
21	91	86	05	87	30	39	19	61	67	69	+ 4.10	18	62	08	22	93	37.34
22	90	85	04	86	30	39	18	61	68	70	11	18	63	10	25	95	38.00
23	89	83	03	85	29	38	18	62	68	71	12	19	65	12	27	98	38.70
24	88	82	02	85	28	38	17	62	69	71	13	20	67	14	29	+ 3.01	39.43
25	86	80	00	84	27	37	16	63	70	72	14	21	69	16	31	04	40.19
26	85	79	+ 2.99	83	26	37	16	63	70	72	15	22	71	18	33	07	40.98
27	84	77	98	82	26	36	15	64	71	73	16	22	72	20	36	09	41.78
28	83	76	97	81	25	36	15	64	71	73	17	23	74	22	38	12	42.57
29	82	74	96	80	24	35	14	65	72	73	18	24	76	24	40	15	43.34
30	81	73	95	79	23	34	13	65	72	74	19	24	77	26	42	18	44.06
31	80	71	94	78	22	33	12	65	72	74	20	25	79	28	44	21	44.72

AUG.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virgin.	Spic. Vir.	Arcturus.
1	+3.28	+2.87	+2.50	+2.20	+2.45	+1.65	+2.18	+1.78	+1.07	+1.90	+1.45	+1.82	+1.20	+1.61	+1.86	+1.84	+2.20	+2.38
2	31	90	53	23	49	68	22	81	09	93	47	84	21	62	85	84	20	37
3	33	93	56	26	53	70	25	83	11	95	48	86	21	62	85	84	19	36
4	36	96	59	29	57	73	28	86	14	98	50	88	22	63	84	83	17	35
5	38	99	62	32	61	76	31	88	16	+2.00	52	90	23	63	83	83	16	33
6	41	+3.03	65	35	64	79	34	91	18	02	54	92	24	63	82	82	15	32
7	43	06	68	38	68	81	37	93	20	04	55	94	25	64	82	82	14	30
8	46	09	71	41	72	84	40	96	22	06	57	96	25	64	81	81	13	29
9	48	12	74	44	76	87	43	99	24	09	59	98	26	64	81	81	12	28
10	50	15	77	47	80	89	46	+2.01	26	11	61	+2.01	27	65	80	80	11	26
11	52	18	80	50	84	92	50	04	29	13	63	03	28	65	80	80	10	25
12	54	21	83	53	88	95	53	07	31	16	65	05	29	66	80	79	09	23
13	56	24	85	56	92	97	56	09	33	18	67	07	29	66	80	79	08	22
14	59	27	88	60	97	+2.00	59	12	35	20	69	09	30	67	79	79	07	20
15	61	30	91	63	+3.01	03	62	15	37	23	71	11	31	67	79	78	06	19
16	63	33	94	66	*05	05	66	18	40	25	73	14	32	68	79	78	05	17
17	65	36	97	69	09	08	69	20	42	28	75	16	33	68	78	77	04	16
18	67	39	+3.00	72	13	11	72	23	44	30	77	18	34	69	78	77	03	14
19	69	42	03	75	17	14	75	26	47	33	79	20	35	70	78	76	02	13
20	71	45	06	80	22	17	79	29	50	36	82	23	37	72	79	76	02	12
21	73	48	09	83	27	20	83	32	52	39	84	26	39	73	79	75	01	11
22	75	51	12	86	31	23	86	35	54	41	86	29	40	74	79	74	00	09
23	77	54	15	89	35	26	89	37	56	44	88	31	41	75	79	73	00	08
24	79	57	18	92	40	29	93	40	59	47	91	33	42	76	78	71	+1.99	07
25	81	60	21	95	44	32	96	43	61	49	93	36	43	77	78	70	98	05
26	83	62	23	98	48	34	99	46	63	52	95	38	45	78	78	69	97	04
27	85	65	26	+3.01	52	37	+3.03	48	66	54	97	41	46	79	78	68	96	02
28	87	68	29	04	56	40	06	51	68	57	99	43	47	80	78	67	95	01
29	88	71	32	07	60	43	09	54	71	60	+2.01	46	48	81	78	68	93	00
30	90	73	34	10	65	46	13	57	74	63	04	48	50	82	78	69	92	+1.99
31	91	75	37	13	69	49	16	60	76	66	06	51	51	84	78	71	90	97

AUG. continued	α Libræ.	α Cor.Bor.	α Serpent.	Antares.	α Herculis	α Ophiuch	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	α Capric.	α Cygni.	α Aquarii.	Fomalh.	α Pegasi.	α Androm.	Polaris.
	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	H. M. s.
1	+2.79	+2.71	+2.93	+3.78	+3.21	+3.33	+3.12	+3.66	+3.74	+3.75	+4.21	+3.25	+3.81	+4.31	+3.47	+3.24	0 . 57 . 45.34
2	78	69	92	77	21	32	11	66	74	75	22	26	83	33	50	27	45.93
3	77	68	91	76	20	31	10	67	74	76	22	27	84	35	52	30	46.53
4	76	66	90	75	19	30	09	67	74	76	23	27	86	38	54	33	47.12
5	75	65	89	74	18	29	08	67	74	77	23	28	87	40	56	36	47.72
6	73	63	87	72	17	28	07	68	75	77	24	28	89	42	58	39	48.35
7	72	62	86	71	16	27	06	68	75	77	24	29	90	44	60	41	49.02
8	71	60	85	70	15	26	05	68	75	78	25	29	92	46	62	44	49.74
9	70	58	84	69	14	25	04	68	75	78	25	29	93	48	64	46	50.45
10	68	56	82	67	12	24	02	67	74	78	25	28	94	49	65	48	51.17
11	67	55	81	66	11	22	01	67	74	77	25	28	95	51	67	51	51.86
12	65	53	79	64	09	21	+2.99	67	74	77	25	28	96	53	68	53	52.53
13	64	51	78	63	08	20	98	67	74	77	25	28	97	54	70	55	53.17
14	63	49	77	62	07	19	97	66	72	77	26	27	98	56	72	57	53.73
15	61	47	75	60	05	18	95	66	72	77	26	27	99	58	73	59	54.25
16	60	46	74	59	04	16	94	66	72	76	26	27	+4.00	60	75	62	54.76
17	58	44	72	57	02	15	92	65	71	76	26	26	01	61	76	64	55.26
18	57	42	71	56	01	14	91	65	71	76	26	26	02	63	78	66	55.77
19	56	40	70	55	00	13	89	65	71	76	26	25	03	64	79	68	56.29
20	56	39	69	55	+2.99	12	88	64	70	76	26	25	04	66	81	71	56.83
21	55	37	68	54	98	11	87	64	70	75	26	24	05	68	82	73	57.42
22	54	35	66	52	96	09	85	63	70	75	25	24	05	69	83	75	58.04
23	53	34	65	51	95	08	83	63	69	74	25	23	06	70	84	76	58.68
24	51	32	63	49	93	06	81	62	69	73	25	22	07	71	86	78	59.28
25	50	30	62	48	92	05	79	62	69	73	24	21	07	72	87	80	59.88
26	49	28	60	46	90	03	78	61	68	72	24	20	08	74	88	82	0 . 58 . 0.43
27	47	26	59	45	89	02	76	61	68	72	23	19	08	75	89	84	0.94
28	46	24	57	43	87	00	74	60	67	71	23	18	09	76	90	86	1.40
29	45	22	56	41	85	+2.98	72	59	67	70	22	17	09	77	91	87	1.80
30	44	20	54	40	84	97	70	58	65	69	22	16	09	78	92	89	2.18
31	42	19	53	38	82	95	69	56	64	68	21	15	10	78	92	90	2.57

SEPT.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis	Spica Vir.	Arcturus.
	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″
1	+3.93	+3.78	+3.40	+3.16	+3.73	+2.52	+3.20	+2.63	+1.79	+2.69	+2.09	+2.54	+1.53	+1.85	+1.78	+1.72	+1.88	+1.96
2	94	80	42	19	77	54	23	65	82	72	11	56	54	86	78	73	87	95
3	95	83	45	23	82	57	26	68	85	75	13	59	56	87	79	74	85	94
4	97	85	48	26	86	60	30	71	88	78	16	62	57	88	79	75	83	93
5	98	88	51	29	90	63	33	74	91	81	19	65	59	90	79	77	81	92
6	+4.00	90	53	32	95	66	37	77	94	84	22	67	60	91	79	78	80	91
7	01	93	56	35	+4.00	69	41	80	97	88	25	70	62	92	80	79	78	90
8	03	96	59	39	05	73	45	84	+2.00	92	28	74	65	94	80	80	80	89
9	05	99	61	43	10	76	49	87	02	95	30	77	66	96	81	81	81	88
10	06	+4.02	63	46	15	79	52	90	05	98	33	80	68	98	81	82	81	87
11	07	04	65	49	19	81	56	93	08	+3.01	35	83	70	+2.00	82	82	82	86
12	08	06	67	52	23	84	59	95	10	04	38	86	71	01	82	82	82	85
13	09	08	70	55	27	87	62	98	13	08	41	89	73	03	83	82	83	83
14	10	10	72	58	31	90	66	+3.01	16	11	43	92	75	04	83	83	83	82
15	11	13	74	61	36	93	69	04	19	14	46	95	77	06	84	83	83	81
16	12	15	76	64	40	96	73	07	21	17	48	98	78	07	84	84	83	80
17	13	17	78	67	44	99	76	10	24	20	51	+3.01	80	09	85	84	83	79
18	14	19	80	70	48	+3.02	79	13	27	23	54	04	82	11	86	85	84	78
19	14	21	83	73	52	05	83	16	30	27	56	07	84	13	86	86	84	77
20	15	23	85	76	56	07	86	19	32	30	59	10	86	14	87	86	84	77
21	16	25	88	79	60	10	89	22	35	33	62	13	88	16	88	87	84	76
22	16	27	90	81	64	13	92	25	38	36	64	16	90	18	88	88	84	75
23	17	30	93	84	69	16	96	28	41	40	67	20	92	20	89	89	84	74
24	18	32	95	87	73	19	99	31	44	43	70	23	94	22	90	90	84	73
25	19	34	98	90	77	21	+4.02	34	46	46	73	26	97	23	91	90	84	73
26	20	36	+4.01	93	81	24	06	37	49	50	76	29	+2.00	25	92	91	85	72
27	21	39	04	97	86	27	10	42	+2.53	54	79	33	03	28	93	93	85	72
28	21	41	07	+4.00	91	30	14	45	56	57	82	37	05	30	94	94	85	72
29	22	43	09	02	95	33	17	48	59	60	85	41	07	32	95	95	85	71
30	22	44	11	05	99	36	21	51	62	64	87	44	10	34	97	96	85	71

SEPT. continued	α Libræ.	α Cor.Bor	α Serpent.	Antares.	α Herculis	α Ophiuch	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	α Capric.	α Cygni.	α Aquarii.	Fomalh.	α Pegasi.	α Androm	Polaris.
		″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	H. M. S.
1	+2.41	+2.17	+2.51	+3.36	+2.80	+2.94	+2.65	+3.55	+3.63	+3.67	+4.20	+3.14	+4.10	+4.79	+3.93	+3.92	0 . 58 . 2.97
2	40	15	50	35	79	92	63	54	62	67	20	12	10	80	94	93	3.40
3	39	13	48	33	77	90	61	53	61	66	19	11	10	81	95	95	3.84
4	38	11	47	31	75	89	59	51	60	65	18	10	10	82	96	96	4.32
5	37	10	45	29	73	87	56	50	59	64	17	09	11	82	96	98	4.82
6	36	08	44	28	72	86	54	49	58	63	17	07	11	83	97	99	5.33
7	35	06	42	26	70	84	52	48	58	62	16	06	11	84	98	+4.01	5.84
8	34	04	42	26	69	83	50	48	57	62	16	05	13	85	99	03	6.28
9	33	02	40	25	67	81	47	47	56	61	15	04	13	86	+4.00	05	6.69
10	32	01	39	23	66	80	45	46	55	60	14	02	12	86	00	06	7.06
11	31	+1.99	37	22	64	78	43	45	54	59	13	00	12	86	01	07	7.38
12	30	97	36	20	62	76	41	44	53	58	12	+2.99	12	86	01	08	7.67
13	29	95	34	18	60	74	38	42	51	56	11	97	12	87	02	09	7.92
14	28	93	33	17	58	72	36	41	50	55	10	95	12	87	02	10	8.17
15	27	92	31	15	57	71	34	40	49	54	09	93	11	87	02	11	8.45
16	26	90	30	14	55	69	31	38	47	52	08	92	11	88	03	12	8.75
17	25	88	28	12	53	67	29	37	46	51	07	90	11	88	03	13	9.10
18	24	86	27	10	51	65	27	35	45	50	06	88	11	88	03	14	9.44
19	23	85	25	09	49	63	24	34	43	48	04	86	10	88	03	14	9.80
20	22	83	24	07	48	62	22	32	42	47	03	84	10	87	03	15	10.18
21	21	82	23	06	46	60	19	31	40	45	02	82	09	87	03	15	10.53
22	21	80	22	04	44	58	17	29	39	44	01	80	09	87	03	16	10.86
23	20	78	20	02	42	56	14	27	37	42	+3.99	78	08	87	02	17	11.11
24	19	77	19	01	40	54	12	26	36	41	98	75	08	87	02	17	11.32
25	18	75	18	+2.99	39	53	10	24	34	39	97	73	07	87	02	18	11.47
26	17	74	16	98	37	51	08	23	33	38	95	71	07	87	02	18	11.60
27	17	73	16	98	36	50	05	22	32	37	95	70	07	87	02	19	11.73
28	16	72	15	97	34	48	03	20	30	35	94	68	06	86	02	19	11.84
29	16	70	14	95	33	47	00	19	29	34	93	66	05	86	02	20	11.96
30	15	69	13	94	31	45	+1.98	17	27	32	92	64	04	86	02	20	12.12

Art. XVI.—i. *Table of Equivalents, for converting Hours, Minutes, and Seconds, into Space.*

For Hours.		For Minutes.						For Seconds.						For Decimal Parts	
H.	Deg.	Min.	Deg.	M.	Min.	Deg.	M.	Sec.	Min.	Sec.	Sec.	Min.	Sec.	Sec.	Sec.
1	15	1	0	15	31	7	45	1	0	15	31	7	45	.1	1.5
2	30	2	0	30	32	8	0	2	0	30	32	8	0	.2	3.0
3	45	3	0	45	33	8	15	3	0	45	33	8	15	.3	4.5
4	60	4	1	0	34	8	30	4	1	0	34	8	30	.4	6.0
5	75	5	1	15	35	8	45	5	1	15	35	8	45	.5	7.5
6	90	6	1	30	36	9	0	6	1	30	36	9	0	.6	9.0
7	105	7	1	45	37	9	15	7	1	45	37	9	15	.7	10.5
8	120	8	2	0	38	9	30	8	2	0	38	9	30	.8	12.0
9	135	9	2	15	39	9	45	9	2	15	39	9	45	.9	13.5
10	150	10	2	30	40	10	0	10	2	30	40	10	0		
11	165	11	2	45	41	10	15	11	2	45	41	10	15	.01	.15
12	180	12	3	0	42	10	30	12	3	0	42	10	30	.02	.30
13	195	13	3	15	43	10	45	13	3	15	43	10	45	.03	.45
14	210	14	3	30	44	11	0	14	3	30	44	11	0	.04	.60
15	225	15	3	45	45	11	15	15	3	45	45	11	15	.05	.75
16	240	16	4	0	46	11	30	16	4	0	46	11	30	.06	.90
17	255	17	4	15	47	11	45	17	4	15	47	11	45	.07	1.05
18	270	18	4	30	48	12	0	18	4	30	48	12	0	.08	1.20
19	285	19	4	45	49	12	15	19	4	45	49	12	15	.09	1.35
20	300	20	5	0	50	12	30	20	5	0	50	12	30		
21	315	21	5	15	51	12	45	21	5	15	51	12	45	.001	.015
22	330	22	5	30	52	13	0	22	5	30	52	13	0	.002	.030
23	345	23	5	45	53	13	15	23	5	45	53	13	15	.003	.045
24	360	24	6	0	54	13	30	24	6	0	54	13	30	.004	.060
		25	6	15	55	13	45	25	6	15	55	13	45	.005	.075
		26	6	30	56	14	0	26	6	30	56	14	0	.006	.090
		27	6	45	57	14	15	27	6	45	57	14	15	.007	.105
		28	7	0	58	14	30	28	7	0	58	14	30	.008	.120
		29	7	15	59	14	45	29	7	15	59	14	45	.009	.135
		30	7	30	60	15	0	30	7	30	60	15	0		

ii. *Table of Equivalents, for converting Degrees, Minutes, and Seconds, into Time.*

For Degrees.						For Minutes.						For Seconds.				For Decimal Parts.	
Deg.	H.	M.	Deg.	H.	M.	Min.	′	″	Min.	′	″	Sec.	″	Sec.	″	Sec.	″
1	0	4	31	2	4	1	0	4	31	2	4	1	0.0667	31	2.0667	.1	.0067
2	0	8	32	2	8	2	0	8	32	2	8	2	0.1333	32	2.1333	.2	.0133
3	0	12	33	2	12	3	0	12	33	2	12	3	0.2000	33	2.2000	.3	.0200
4	0	16	34	2	16	4	0	16	34	2	16	4	0.2667	34	2.2667	.4	.0267
5	0	20	35	2	20	5	0	20	35	2	20	5	0.3333	35	2.3333	.5	.0333
6	0	24	36	2	24	6	0	24	36	2	24	6	0.4000	36	2.4000	.6	.0400
7	0	28	37	2	28	7	0	28	37	2	28	7	0.4667	37	2.4667	.7	.0467
8	0	32	38	2	32	8	0	32	38	2	32	8	0.5333	38	2.5333	.8	.0533
9	0	36	39	2	36	9	0	36	39	2	36	9	0.6000	39	2.6000	.9	.0600
10	0	40	40	2	40	10	0	40	40	2	40	10	0.6667	40	2.6667		
11	0	44	41	2	44	11	0	44	41	2	44	11	0.7333	41	2.7333	.01	.0007
12	0	48	42	2	48	12	0	48	42	2	48	12	0.8000	42	2.8000	.02	.0013
13	0	52	43	2	52	13	0	52	43	2	52	13	0.8667	43	2.8667	.03	.0020
14	0	56	44	2	56	14	0	56	44	2	56	14	0.9333	44	2.9333	.04	.0027
15	1	0	45	3	0	15	1	0	45	3	0	15	1.0000	45	3.0000	.05	.0033
16	1	4	46	3	4	16	1	4	46	3	4	16	1.0667	46	3.0667	.06	.0040
17	1	8	47	3	8	17	1	8	47	3	8	17	1.1333	47	3.1333	.07	.0047
18	1	12	48	3	12	18	1	12	48	3	12	18	1.2000	48	3.2000	.08	.0053
19	1	16	49	3	16	19	1	16	49	3	16	19	1.2667	49	3.2667	.09	.0060
20	1	20	50	3	20	20	1	20	50	3	20	20	1.3333	50	3.3333		
21	1	24	100	6	40	21	1	24	51	3	24	21	1.4000	51	3.4000	.001	.0001
22	1	28	150	10	0	22	1	28	52	3	28	22	1.4667	52	3.4667	.002	.0001
23	1	32	200	13	20	23	1	32	53	3	32	23	1.5333	53	3.5333	.003	.0002
24	1	36	250	16	40	24	1	36	54	3	36	24	1.6000	54	3.6000	.004	.0003
25	1	40	300	20	0	25	1	40	55	3	40	25	1.6667	55	3.6667	.005	.0003
26	1	44	350	23	20	26	1	44	56	3	44	26	1.7333	56	3.7333	.006	.0004
27	1	48	360	24	0	27	1	48	57	3	48	27	1.8000	57	3.8000	.007	.0005
28	1	52				28	1	52	58	3	52	28	1.8667	58	3.8667	.008	.0005
29	1	56				29	1	56	59	3	56	29	1.9333	59	3.9333	.009	.0006
30	2	0				30	2	0	60	4	0	30	2.0000	60	4.0000		

Art. XVII. PROGRESS OF FOREIGN SCIENCE

I. Chemical Science.

i. *Analysis of the Mineral and Thermal Waters of Saint Nectaire, Department of the Puy-de-Dome. By* M. P. Berthier, *Engineer of Mines.*

	Anhydrous Salts.	Crystalline Salts.
Free carbonic acid	0.000736	0.000736
Bi-carbonate of soda	0.002833	0.003150
Muriate of soda	0.002420	0.002420
Sulphate of soda	0.000156	0.000350
Carbonate of lime	0.000440	0.000440
Carbonate of magnesia	0.000240	0.000240
Silica	0.000100	0.000100
Oxide of iron	0.000014	0.000014
	0.006203	0.006714

Or, supposing the soda in the state of subcarbonate—

	Anhydrous Salts.	Crystalline Salts.
Free carbonic acid	0.001545	0.001545
Subcarbonate of soda	0.002024	0.005419
Muriate of soda	0.002420	0.002420
Sulphate of soda	0.000156	0.000350
Carbonate of lime, &c.	0.000794	0.000794
	0.005394	0.008983

There are, therefore, few mineral waters so rich in alkaline salts, as those of Saint Nectaire. They are, however, much inferior in this respect to the waters of Vichy, in Auvergne, which contain 0.0038 of anhydrous carbonate of soda, whilst there is only 0.002 in those of Saint Nectaire.—*Ann. de Ch. et de Phys.*, xix. 122.

On the Subnitrates, and Super-nitrates. By M. P. Grouvelle.—Berzelius has given the analysis of the subnitrates of lead and copper, in his *Essay on the Theory of Chemical Proportions.* It thence appears that in the nitrates which he examined, the oxygen of the nitric acid is to the oxygen of the oxide, as 5 to 1, 2, 3, 6; but he does not seem to have studied any other subnitrates. 2.347 gr. of a subnitrate of zinc were found to consist of—

Oxide	1.948	4 atoms	81.69
Acid	0.328	1 ——	13.75
Water	0.109	2 ——	4.56
	2.385		100.00

Subnitrate of iron, prepared by a powerful evaporation, and dried in a glass tube, till it was on the verge of decomposition, consisted of—

Tritoxide of iron	1.543	4 atoms	81.26
Nitric acid	0.267	1 ——	14.06
Water	0.088	2 ——	4.68
	1.198		100.00

Subnitrate of bismuth. 2.827 parts of the salt, precipitated by water, and dried with precaution on the fire, were found to consist of—

Oxide	2 atoms	2.405
Acid	1 ——	0.413
		2.818

5.348 of the same subnitrate, dried *in vacuo*, by means of sulphuric acid, left 4.353 of oxide after calcination; they consisted of—

Oxide	4.353	2 atoms	81.37
Acid	0.747	1 ——	13.97
Water	0.248	2 ——	4.66
			100.00

Sub-protonitrate of mercury, precipitated by water. Its black oxide was separated by potash in excess, collected on a filter and weighed. 8.979 parts yielded 7.947 of oxide.

Oxide of mercury	7.947	2 atoms	88.60
Acid	1.022	1 ——	11.40

Sub-deutonitrate of mercury, precipitated by water, gave a compound of—

Oxide	3.901	2 atoms	88.97
Acid	0.484	1 ——	11.03
	4.385		100.00

When it is precipitated, by alkali not in excess, this sub-deutonitrate cannot be washed, because water decomposes it instantly and completely; and even the preceding sub-deutonitrate is destroyed by boiling water, which carries off the whole of the acid, with a little oxide. It becomes, therefore, necessary to filter the sub-deutonitrate separated by the potash, to compress it between papers, and dry it *in vacuo*. It was then of a fine yellow colour, and did not decompose in the air. Two analyses gave more acid than was requisite for the constitution of two atoms of oxide and one of acid. But as it still retained much neutral nitrate, it is almost certain, says M. Grouvelle, that its composition was as follows:—

2 atoms oxide . . . 5.910, quantity obtained.
1 —— acid . . . 0.703

0.613

6.8 parts had been employed.

" It follows," says he, " from these analyses, that in the nitrates hitherto examined, the oxygen of the acid is to that of the oxide, as 5 to 1, 2, 3, 4, 6, 7, 8."

Of the acidulous nitrates. As we now know the composition of the subnitrates of bismuth and mercury, in order to learn what takes place when the neutral nitrates are decomposed by water, we must examine the proportion between the quantity of oxide precipitated in the state of subnitrate, and that which remains in solution.

10.142 gr. of crystallized nitrate of bismuth, dried at the fire, to expel as much as possible the excess of acid, were treated with a great quantity of water. There was thus obtained—

5.493 gr. of subnitrate, retaining still a little humidity besides the water of combination;
10.142 of neutral nitrate, contain oxide 6.014 3 atoms.
5.493 of subnitrate . . . . . 4.471 2 ——

3 atoms of neutral nitrate have then let fall 2 atoms of oxide and 1 atom of acid. There remained in solution 1 atom of oxide and 5 atoms of acid.

16.319 of protonitrate of mercury, well dried, gave 2.491 of subnitrate, containing 2.226 of oxide.

These 16.319 contain 12.972 of oxide. Hence 12 atoms of neutral nitrate afforded 1 atom of subnitrate. The water retained 10 atoms of oxide and 11 atoms of acid.

17.950 of deuto-nitrate of mercury, containing 12 of oxide, yielded with cold water 4.622 of subnitrate, containing 4.112 of oxide.

2 atoms of oxide 4.112 precipitated;
4 . . . . . 8.224 left in the solution.

Total 12.336

There has been deposited, therefore, 1 atom of sub-nitrate, while 4 atoms of oxide and 11 of acid have been dissolved.

We perceive that the acidulous nitrate of bismuth is formed of oxide 1 atom, acid 5 atoms; that of protoxide of mercury, of oxide 10 atoms, acid 11 atoms; that of the deutoxide, of oxide 4 atoms, acid 11 atoms; that is to say, of a quantity of acid in excess over the neutral nitrate, which is by no means proportional to that which constitutes the neutral salt or sub-salt; for they would all be composed as follows:—

Nitrate of Bismuth.

	Neutral.	Acidulous.
Oxide	1 atom	1 atom
Acid	2 ——	5 ——

Proto-nitrate of Mercury.

Oxide	1 atom	10 atoms
Acid	1 ——	11 ——

Deuto-nitrate of Mercury.

Oxide	1 atom	4 atoms
Acid	2 ——	11 ——

If it be considered, moreover, that water alone, in saturating the nitric acid, can decompose the sub-deutonitrate of mercury, it will appear evident that it is not acidulous nitrates which are here formed, but a combination of neutral nitrate, water and acid; where the acid acting at once both on the water and on the neutral nitrate, prevents this from being subsequently decomposed by a new quantity of water.

On the above statements of M. Grouvelle, we have only to remark, that the formation of his salts, does not seem sufficiently determinate to warrant all his atomical inferences. We do not believe, for example, in the existence of a compound of ten atoms of oxide of mercury, and eleven atoms of nitric acid.—*Annales de Ch. et de Phys.* xix. 137.

Analysis of a Salivary Calculus of a Horse, and of the Saliva of the same Animal, by M. J. L. Lassaigne.—This concretion had a cylindrical form, resembling that of an elongated ellipsoid. Its length was forty-seven millimetres, and its diameter eighteen. It was as hard as marble. It consisted of concentric coats, without any central nucleus of foreign matter. Its constituents were

Carbonate of lime	84
Phosphate of lime	3
Animal matter	9
Water	3
Loss	1
	100

This result, along with the others already known, demonstrate that the salivary *calculi* found in herbivorous animals, are generally different from those which occur in man, which consist of phosphate of lime, with a little animal matter.

The saliva of the horse, when evaporated at a moderate heat, leaves 3½ parts in the hundred of fixed principles, which are composed of the following ingredients;

Animal matter soluble in alcohol;
Animal matter soluble in water;
Albumen;
Traces of mucus;
Muriates of soda and potash;
Free soda;
Carbonate of lime;
Phosphate of lime;

The saliva of the horse thus presents some differences from that of man, analyzed by M. Berzelius. It contains more albumen and carbonate of lime, and much less mucus. This anomaly may, perhaps, be ascribed to the different mode of obtaining the saliva for analysis. That of the horse was procured by dissecting with care the salivary canal, and receiving the liquid, which flowed out in abundance while the animal was eating. The quantity obtained by this operation, might be estimated at about half a litre.—*Ann. de Ch. et de Phys.* xix. 174.

On the Triple Chloride of Gold and Sodium, by M. Figuier, apothecary at Montpellier.—This is the medicinal preparation, which Dr. Chrestien, of Montpellier, has been in the habit of employing. If to a solution of chloride of gold, made with two ounces of this metal, we add four drachms (about 300 grains troy) of decrepitated sea salt, and evaporate, we obtain a salt perfectly crystalline, containing always the same proportions of gold, which, far from attracting humidity, like the simple chloride of gold, is on the contrary nearly inalterable in the atmosphere, and not liable to change its nature by repeated crystallizations. M. Figuier determined the quantity of gold in this salt, by throwing it down with sulphuretted hydrogen; he found the muriate of soda present, by evaporating to dryness the supernatant liquid of the preceding experiment; and thirdly, he ascertained the proportion of chlorine, by nitrate of silver. By this process he found, that the chloride of gold and sodium contained,

Chloride of gold	69.3
Chloride of sodium	14.1
Water	16.6
	100.0

He then shews that calling the atom of gold 248.6 with Berzelius, a triple chloride which would be formed of one atom of chloride of gold, one atom of chloride of sodium, and eight atoms of water, would have for its constituents,

Chloride of gold, one atom	380	70
Chloride of sodium, one atom	73	13.4
Water, 8 atoms	11.2	16.6
		100.0

Ann. de Ch. et de Phys. xix. 177.

On a new Compound formed by mixing together a solution of Cyanide of Mercury with a solution of Iodide of Potassium by M. Calliot. —In trying to detect the presence of cyanide of mercury, by the iodide of potassium, he was much surprised to see form in the liquid a multitude of white pearly crystals, instead of a precipitate of deutiodide of mercury, as he expected. After having washed these crystals, he dissolved them in water, and re-crystallized the solution. He obtained large plates, thin and brilliant, unalterable in the air, inodorous in the dry state, but having a smell like that of bitter almonds when in solution; soluble in sixteen times their weight of water, at the ordinary temperature, but requiring much less hot water for their solution. They are also soluble in about ninety-six parts of alcohol, at 34° (0.847.).

This new compound, at a temperature incapable of destroying it, loses nothing of its weight, or its lustre. It is of consequence, probably anhydrous. When more strongly heated, it is decomposed, and gives for products, cyanogen, mercury, and a greenish yellow vapour, mingled with protiodide of mercury. The iodide of potassium which remains fixed is blackened by a little finely-divided charcoal. Put successively in contact with the strongest and the weakest acids, such as the benzoic, camphoric and arsenious, it is converted into deutiodide of mercury, and hydrocyanic acid is disengaged. This acid itself, and carbonic acid, produce no effect on the compound. By sulphuretted hydrogen, black sulphuret of mercury is thrown down, and hydrocyanic acid is disengaged.

The hydro-sulphurets produce on the solution of this body, a black precipitate; the salts of lead, a yellow precipitate of iodide of lead, and the salts of deutoxide of mercury, a red precipitate of deutiodide of mercury. Chlorine and the bi-chloride of mercury determine in it a red precipitate soluble in an excess of the solution. Iodine dissolves in it, insomuch the greater quantity, as its solution is the more concentrated. Potash, soda, and ammonia, whether free or combined with an acid, produce no decomposition of it.

M. Caillot had regarded this compound as formed of one proportion of cyanide of mercury, and one proportion of iodide of potassium; but having added sulphuric acid in slight excess to the solution, he found in the liquid, resting over the abundant precipitate of deutoxide of mercury which took place, a small quantity of cyanide of mercury. It remains, therefore, to make the analysis of it, in order to learn its composition. He concludes, by stating that 24 parts treated by hydrosulphuret of soda, afforded nearly 13 of sulphuret of mercury.—*Ann. de Ch. et de Phys.* xix. p. 220.

On the Zëine of Maize.—The *zëine* of John Gorham, is obtained from Indian corn, by infusing it in water, filtering, and treating with alcohol the matter insoluble in the former liquid, and evaporating the alcoholic solution. We thus obtain a yellow substance having the appearance of wax; it is soft, ductile, tough, elastic, insipid, nearly void of smell, and denser than water. When heated, it swells, becomes brown, exhales the odour of burned bread mixed with an animal smell, and leaves a bulky charcoal. It affords no ammonia. Insoluble in water, it dissolves readily in alcohol, oil of turpentine, sulphuric ether, and partially in the mineral acids, and caustic alkalis. It is insoluble in the fixed oils, but may be united with the resins.

Though different from all known vegetable products, it approaches in its nature somewhat to gluten, from which, however, it is distinguished by the absence of azote; by its permanence; for it does not change its nature, or become spoiled in the air; and, finally, by its solubility in alcohol. It resembles the resins in this respect, since it dissolves like them in the volatile oils, &c. It is inflammable, and consists of carbon, hydrogen, and oxygen. It is very readily extracted from the maize, by digestion for a few hours in hot alcohol. The filtered liquid, yields, on evaporation, pure zëine.

On the Hop. By M. M. Payen and A. Chevalier.—It appears that M. Planche, some time ago, ascertained that the three active ingredients of hop, the oil, resin, and bitter principle reside in the brilliant yellow grains scattered over the calicinal scales of the cones, which serve as their envelope. Dr. Yves, of New York, and the authors of the present memoir, now confirm this position. This matter, when insulated, is of a golden yellow colour, in little grains, formed of an impalpable powder, without consistence, which attaches itself to the fingers and renders them rough; it has a penetrating aromatic odour. 200 grammes of this substance being put into a retort, with 300 grammes of distilled water, the mixtnre was subjected to distillation, and afforded water and oil of an odour entirely similar to that of this yellow matter, but much more penetrating, narcotic, and very acrid in the throat. It was not easy to determine the weight of this essential oil, although it was in considerable quantity, because it is very volatile, soluble in a great measure in water, and adhering to the sides of the globular receiver. However, from the weight of what they could collect, and by making an approximate estimate of the rest, they imagined that the total amount of the oil was 4 grammes, or 2 *per cent.* of the yellow matter employed; and as this yellow matter is contained in hop, in the proportion of $\frac{1}{10}$, it follows that the hop contains about 0.002 of essential oil.

The water, over which the oil floated, had the same flavour as

the oil itself, but less strong. After drawing it off, and leaving it for a few days, it became alkaline, and its acrimony disappeared. They found that the alkaline properties were owing to a subacetate of ammonia. The following are the ingredients they extracted from the 200 grains of this yellow substance:

1 Water;
2 Essential oil;
3 Carbonic acid;
4 Subacetate of ammonia;
5 Traces of osmazome;
6 Traces of fatty matter;
7 Gum;
8 Malic acid;
9 Malate of lime;
10 Bitter matter, 25 grammes;
11 A well characterized resin 105 grammes;
12 Silica 8
13 Traces of carbonate, muriate, and sulphate of potash;
14 Carbonate and phosphate of lime;
15 Oxide of iron and traces of sulphur.

The resin is of a golden yellow colour, passing to orange red on exposure to the air. The bitter matter, when dried, is white, yellowish, attracts slightly the humidity of the air; put in the mouth it causes, as its name indicates, a bitter taste; taken inwardly in very small quantity, it destroyed the digestive faculties, depriving the person of all appetite for food; an action which lasted from eight to ten hours. It occasions no narcotic feeling, as the essential oil appears to do; it is soluble in water, ether, alcohol, communicating to these liquids its bitterness. With re-agents it presents the following phenomena:

With acetate of lead . .	no change,
Subacetate of lead . . .	0
Nitrate of cobalt . . .	slightly troubled,
Muriate of platinum . .	a light precipitate, insoluble in even a large quantity of water,
Nitrate of silver	a slight mottling,
Infusion of nut galls . .	0
Nitrate of mercury . . .	mottling,
Muriate of tin	slightly turbid,
Bichloride of mercury . .	white precipitate,
Sulphate of iron	slight turbidity,
Nitrate of copper . . .	slight, flocky precipitate.

The resin is soluble in alcohol and ether, colouring these *menstrua* to a golden yellow. The solution yields, by evaporation, a resin which, when detached from the capsule, is in fine

yellow scales, of a perfect transparency. Put in the mouth, it imparts to the palate a bitter taste; distilled water boiled on it acquires this taste, without any colour. With the weak acids it experiences no alteration. It is dissolved by the alkalis; and the acids precipitate it from this solution. The above results were obtained apparently from English hops.

According to Doctor Yves (*Annals of Phil.*, March, 1821,) 120 grains of the yellow grains, treated with different menstrua, afford—

Tannin	5 grains
Extractive matter	10
Bitter principle	10
Wax	12
Resin	36
Fibrous or ligneous residuum .	46
	119

M. Planche, one of the Editors of the *Journ. de Pharmacie*, in commenting on the above Memoirs on the Hop, thinks himself warranted, from his own experiments, to consider the analysis of M. Payen more exact than that of Doctor Yves. We are promised in a future Number some remarks on the medicinal properties of the above yellow matter of the hop.

On the volatile Oil of Bitter Almonds as a Poison. By M. Vogel, *of Munich.*—To deprive the oil obtained from bitter almonds by distillation, of its hydro-cyanic acid, he agitated it with a concentrated solution of potash, and distilled it to dryness. The oil volatilized with the water, and the residuum in the retort contained cyanide of potassium. To be certain that the oil was entirely deprived of all its hydro-cyanic acid, he distilled it anew with potash; but this time the residuum contained no cyanide of potassium.

The volatile oil of bitter almonds, thus purified, is without colour, and heavier than water. Its taste is extremely acrid and burning; it crystallizes rapidly by contact of air; it dissolves easily in alcohol and ether, but only in very small quantity in water. The flame of its combustion is very brilliant, and accompanied with much smoke.

In order to find whether this oil, freed from its hydro-cyanic acid, was still poisonous, M. Vogel put a drop of it on the tongue of a sparrow; it died, after violent convulsions, in a few seconds. He poisoned a dog, two months old, with four drops of it. He hence infers, that this volatile oil, well purified, produces on animals deleterious effects, analogous to those of the hydro-cyanic acid, although in a feebler degree.

Analytical examination of two species of Flour, designated under the names of Wheat Flour of Odessa, and French Wheat Flour. By M. Henry.—The Odessa flour was of a dirty yellowish hue, with scarcely any direct taste, but leaving in the mouth a powdery after-taste; its smell was not unpleasant, yet approaching to that of dust. It was somewhat rough to the touch, of little unctuosity, and contained many small yellowish points. The French flour was of a fine white, a fresh smell, an agreeable taste, and softer than the other to the touch.

A paste was made with each of these flours and water. The Odessa specimen absorbed 60 parts of water *per cent.*; the French 45 parts. The Odessa paste had a dirty yellowish aspect, it was elastic and tough; when bruised between the teeth, it developed a bitter taste. The paste of the French flour was greyish white, elastic, less tenacious than the former, and of a sweet taste.

These doughs were separately washed under a slender stream of water, kneading them continually in the hands. By this means all the gluten was obtained, which was well washed, weighed, dried in a stove, heated to 40° centigrade (104° Fahrenheit), and weighed anew. It lost thus two-thirds of its weight. The gluten obtained from the Odessa wheat, weight 36.5 when fresh, and 12 in the dry state. This gluten had a greyish aspect, was very elastic, very tough, and appeared to be of a very good quality. The gluten extracted from the French flour weighed 24.5 fresh, and 8 dry; it was greyish, elastic, and tough. It remained in water without changing, for a much longer time than that of the Odessa flour.

The water of the washings of the flours, containing the starch, was filtered in order to separate this principle, which washed, dried, and weighed, presented the following characters for each flour:

The starch of the Odessa wheat was greyish white, rough to the touch, and gritty under the teeth; it weighed 66. That of the French wheat was more decidedly white, and less harsh to the touch; it weighed 70, the water of the washings, afforded no blue precipitate with tincture of iodine; it was slightly opaque. The water from the Odessa wheat had a bitterish taste, not found in that of the French. This water, exposed to a gentle heat, deposited a matter which was recognised to be albumen. That of the Odessa washings evaporated to dryness, at the heat of a salt-water bath, afforded a residuum of a reddish-brown, and the other of a yellowish-brown. These *residua* were viscid, and a little saccharine. That of the Odessa grain was also slightly bitter. Treated with water to separate the albumen, the liquid was evaporated anew to the consistence of an extract.

The extractive matter was now subjected to the action of alcohol at 40° (8.817,) in order to separate the portion of sugar which it contained. The quantity of sugar furnished by each was nearly the same; but that of the Odessa specimen was coloured and faintly bitter.

The residuum insoluble in alcohol, was digested with water, and concentrated. It became viscid, was somewhat whitish, without any marked taste, and a little coloured. Silicated potash yielded a precipitate, indicating the presence of gum. To determine what salts are contained in these flours, they took 100 parts of each, and calcined them slightly in a platinum crucible. The carbonaceous residuum was pulverized, and treated with boiling water; this filtered and evaporated to dryness, gave for each flour about 0.15 of saline matter, and a minute portion of silica.

On examining the bread manufactured with these two kinds of flour, the French was found to be the sweetest, but the other kept longer fresh.—*Jour. de Pharm.* Feb. 1822.

On Carbon considered as a discolouring substance, by M. A. Bussy.—This memoir, to which the Pharmaceutical Society of Paris has very properly adjudged the first prize, is preceded by general considerations on colouring matters, which the limits of our Journal do not permit us to detail. M. Bussy, in order to shew that the action of a chemical body varies much according to the physical state in which it occurs, quotes the fact that gelatinous alumina deprives of colour decoctions of almost all dyewoods, whilst it has no action on them, when it has acquired cohesion by desiccation. On adding likewise to a liquor, a solution of acetate of lead, we destroy the colour by occasioning a precipitate, which carries with it a portion of the colouring matter, whilst the same precipitate in the dry state would exercise no action on it.

M. Bussy's first chapter treats of the manner of comparing together the discolouring or blanching powers of different charcoals. The solution of indigo in sulphuric acid appears to him the best means to be employed for establishing in an exact and comparative manner, the discolouring power of different charcoals. 1st, Because we can always know the exact quantity of colouring matter on which we act, from the weights of indigo dissolved, and water of dilution. 2d, Because the marked hue of indigo does not suffer us to hesitate concerning the blanching of the liquor, when it occurs. 3d, Because indigo is less susceptible than any other matter of being altered by light, heat, *&c.*, which might exercise some influence on the results.

It might, indeed, be properly objected, that the acid of the solution may act upon the substances foreign to the charcoal. To avoid this inconvenience, M. Bussy took a solution made

neutral in the following way:—Into an acid solution of indigo, he put a certain quantity of wool; when it had taken up all the colour of which it was capable, he withdrew it, washed it with cold water, to remove all the loosely-adhering indigo; after which he boiled it in water containing an excessively small quantity of potash; merely what he supposed necessary to saturate the acid which the wool might still retain. In this manner a neutral solution was obtained, whose proportion of indigo could be ascertained; for it is known from the very exact experiments of M. Welter, that 100 parts of chlorine in weight destroy the colour of 226 parts of indigo. Hence in taking a solution of chlorine, whose proportions are known, we can determine what is the quantity of the solution of indigo which a known portion of the first decomposes. It was in this way that M. Bussy learned that the solution which served for his trials, contained one thousandth of its weight of indigo.

To try a charcoal with this solution, he took a certain quantity, which he put into a phial, in contact with a known quantity of the charcoal; he heated slightly, which hastens somewhat the discoloration, and he added the test-liquor till the charcoal ceased to discolour it. The discoloration also takes place in the cold, but more slowly.

In his second chapter he inquires what among the different substances, contained in charcoal, are those which act efficaciously in discoloration. He began by trying different species of charcoal obtained by calcination in close vessels, at a heat sufficiently strong to get rid as much as possible of the gaseous bodies which it usually evolves. He thus calcined wood, starch, gelatine, gum, blood, coal. All these species of carbon were more or less hard, friable, brilliant, and *did not sensibly discolour the test-liquor.* He next examined bone black, usually employed for the clarification of sugar in France; and he examined particularly the charcoal, known under the name of the charcoal of Prussian blue, which is produced by the calcination of animal matters along with potash. To procure this last product he took dried ox-blood, which he calcined twice over with its own weight of subcarbonate of potash, at a temperature a little under a dull red; he obtained a spongy mass, which, after being sufficiently washed with boiling water, to separate every thing soluble which it contained, left a charcoal of a dull black hue, excessively light and spongy, whose discolouring power, compared to that of bone-black, was as 40 to 1. Astonished at so great a difference, he wished to see if the charcoal had not contracted some new combination, or if it owed this exaltation of property to the presence of a foreign body, and particularly to potash. After having washed it as well as possible in boiling water, which removed the ferro-prussiate of potash, as well as the sulphuret and carbonate of this

alkali, the residuum was treated with muriatic acid, which occasioned an effervescence due to the decomposition of a little sulphuret of iron; and the acid being boiled on the charcoal, carried off the phosphate of lime, with a little lime and iron. This charcoal so treated, discoloured still very well; even better than before. It was therefore improbable that this charcoal owed its property to the presence of potash, unless indeed there had been formed some alloy which could resist the action of muriatic acid. To ascertain this point, he calcined the charcoal which had been treated with the acid, but the ashes were not alkaline, and contained no salt of potash; they were a mixture of oxide of iron and silica, forming a twelfth part of the weight of the charcoal calcined.

It is then in the residuum of the action of muriatic acid that the discolouring property resides, and this residuum is composed of charcoal, and iron in the state of a carburet probably, since it is not acted on by muriatic acid, a little silica, which is only accidental, and azote. To determine what might be the influence of the azote, he took calcined blood alone; 3 decigrammes of this blood, burned with peroxide of copper, yielded 12 of azote for 100 in weight of the charcoal. The proportion of azote is very variable; we may even fail to obtain any, if the charcoal has been strongly enough heated; but in all these cases this charcoal is hard, brilliant, and *does not discolour*. That resulting from the calcination of blood with potash, tried by the oxide of copper, contains still a certain quantity of azote, also variable, but much less than the first; but as it is one of the characters of azotized carbon to furnish hydro-cyanate of potash when we calcine it with this alkali, he conceived that by treating this charcoal with a new quantity of potash, he could remove from it still a portion of azote; he treated it anew with potash, and the quantity of azote which after the first calcination was 5 *per cent.*, was reduced after the second to 2; and at the third the charcoal contained no longer a sensible quantity of it. Now since this reiterated action of the potash merely augmented the discolouring property of the charcoal to such a degree, that after the last calcination it was equal to 50, that of bone-black being 1, *it follows that azote is without effect in discoloration; and consequently, it is not to this body that animal charcoal owes its property.*

It remained to know if the discolouring property was not owing to the combination of charcoal and iron, a combination which resists, as we have seen, the action of boiling muriatic acid; but whose presence cannot be denied, since we always find iron in the residuum of the combustion of charcoal. This quantity of residuary iron, however, is not constant, which shews that the combination is not in any fixed proportion. It is observed, that the quantity of iron is so much greater as the heat was stronger, at which the potash and blood were calcined; but

on the contrary, if we conduct the heat properly, and repeat the calcination with the potash a sufficient number of times, the residuum will contain but a very minute quantity of iron.

The cause of this difference is easily perceived. We know that when we treat animal matters, and especially blood, with potash, we obtain ferro-prussiate of potash, since on this product is founded the manufacture of Prussian Blue; but ferro-prussiate of potash contains hydrogen, carbon, iron, and azote. The three last substances are derived from the elements of the blood, which combine together so as to give birth to the ferro-prussic acid; consequently the more we shall favour the formation of the ferroprussiate of potash, the more iron we shall carry off from the blood.

But if we apply all at once a too violent heat, then no more ferro-prussiate of potash is formed; for this salt is decomposable at a very high temperature, and gives as products of its decomposition a little hydrocyanate of potash, and a solid compound of iron and charcoal. In this case the azote of the blood is dissipated, and the iron remains entire, combined with the charcoal. M. Bussy succeeded by several successive calcinations of blood with potash, to deprive it of all the iron which it contained. The greatest difficulty experienced to attain this point depends upon this, that the azote abandons more easily the charcoal than the iron does, and that when we have arrived at the point where the charcoal contains no more azote, potash is then without action on the iron. But if at this period we add blood-charcoal, or any animal substance containing much azote, as an empyreumatic animal oil, then we succeed readily in carrying off the residuary portion of iron, and we obtain a charcoal which burns without residuum. Albumen and gelatine, substances which contain much less iron than blood, are very readily brought to the point of containing none of it. For this purpose it is sufficient to dry them, to reduce them into powder, and to calcine them with the potash of commerce. We must likewise take care after every calcination to treat the residuum at first with water, then with muriatic acid, which removes every time a small portion of iron, which is probably not combined with charcoal.

Since in all these successive treatments to which we subject charcoal to deprive it of foreign substances, it loses none of its discolouring power, we must conclude that this property resides essentially in charcoal, but that it is developed in virtue of the physical circumstances in which it is placed. This conclusion which rigorously follows from the experiments just adduced, is supported by a multitude of others, of which a few may be detailed; and which were made chiefly with the intention of shewing that the discolouring property of charcoal varied according as the substances with which we calcine it, act with more or less energy on it, and on the foreign bodies which it contains.

We have seen that blood, calcined alone, yielded a charcoal, which does not discolour, because its particles contract so strong an aggregation, that they are not susceptible of combining with the colouring matters. But if instead of calcining the blood alone, we calcine it mixed with an inert substance, with phosphate of lime for example, which does nothing but oppose the aggregation of the particles, we obtain a charcoal susceptible of discolouring, and whose blanching power will be represented by 12, assuming always for unity, the blanching power of bone-black. If we calcine blood with very fine carbonate of lime, its discolouring property becomes 18; finally, if we treat it with potash, it may even rise to 50. It is needless to say, that in these different trials, the charcoal was deprived of the potash, chalk, or phosphate of lime, with which it had been calcined.

We may easily understand all these results, by considering that the first substance, phosphate of lime, acts simply by operating the division, nearly mechanical, of the charcoal; the second acts already with a little more energy, for we observe that the water of edulcoration contains a small quantity of ferro-prussiate of lime; finally, the potash not only attenuates the particles of the charcoal, but by combining with all the foreign principles which it contains, it must leave it in a certain state of porosity, which M. Bussy regards as more favourable to discoloration than division itself.

In order to ascertain that charcoal truly enjoys the property of discolouring, independently of the substances which are made to act upon it, M. Bussy sought to procure pure charcoal, by means of the decomposition of subcarbonate of soda with phosphorus, and he succeeded in obtaining a sufficient quantity to determine its discolouring power, which he found to be equal to 12.

He next tried lamp-black; but in its natural state it contains resinous matter, which prevents us from regarding it as pure charcoal. To make it such, we must calcine it anew. In this state it is capable of discolouring; its blanching power being expressed by 4; and although it is obviously less than that of the preceding, it is enough to shew that the property resides in charcoal, and that it may vary from 4 to 12, by the mere effect of physical circumstances, very difficult to appreciate; for it could not be easy to establish a perceptible difference between these two charcoals. If instead of heating the lamp-black by itself, we mingle it exactly with 15 or 20 times its weight of pure carbonate of potash (that from calcined tartar), and expose it to a very strong heat in a platina crucible, it is observed that the mass becomes hard, compact, experiencing incipient fusion in the parts contiguous to the crucible. If we dissolve this mass in water, in order to separate all the pot-

ash from it, the charcoal which remains has a discolouring power, expressed by 15; and this charcoal contains no foreign body, for it burns away without leaving any residuum. Besides, we cannot here suppose an alloy of charcoal with potassium, since these kinds of alloys are decomposed by water; whilst the discolouring property, continues still in all its intensity, after the action of muriatic acid. We are, however, led to conclude, that potash may at a high temperature act upon finely divided charcoal, so as to change its physical constitution; and that this change is the sole source of the increase of its blanching power, since it has contracted no combination with it. M. Bussy attempted to substitute caustic potash for the subcarbonate, to see if the effect would be greater; but as the caustic potash contains much water, the charcoal decomposed this, and was thus entirely transformed into carburetted hydrogen gas, and into carbonic acid gas, which combined with the potash.

We now readily perceive how by calcining a vegetable substance with potash, we come to obtain a discolouring charcoal; and in fact, by calcining starch with four parts of potash, a charcoal is obtained, whose blanching property was expressed by 10. The charcoal resulting from the calcination of the acetate of potash, possesses a discolouring power of 3. And we see also, that if the discolouring force of these charcoals is not so great as that of the charcoal obtained from the calcination of animal matters, this depends on the circumstance, that in these last, there are two causes, which concur to augment the blanching power; 1. the action of the potash on the charcoal itself; and 2. the abstraction which it effects of the foreign matters, such as the iron and azote, whilst in the vegetable matters, the first effect is the only one which occurs.

Animal matters themselves do not all yield a charcoal equally good for discolouring; but it is so much the better, as the potash has combined with a larger quantity of the principles of the animal substance; hence, the more prussiate of potash a body affords, when calcined with this alkali, the better will its charcoal discolour. Gelatine and albumen, which yield much less Prussian blue than the blood does, furnish a charcoal, whose discolouring force is from 36 to 40, whilst that of blood amounts even to 50.

Let us now endeavour to explain the mode of action of bone-charcoal, which is most in use, at the same time that it is one of the most complex in its composition. This charcoal, such as we find it in commerce, varies in its composition; but in general we find it to contain—

Phosphate of lime Carbonate of lime Sulphuret of lime Sulphuret of iron Oxide of iron . .	 88
Iron in the state of a silicated carburet	2
Charcoal, containing 6 or 7 *per cent.* of azote	10
	100

When we treat this charcoal with muriatic acid, it leaves a residuum of about twelve per cent., which is a mixture of azotized carbon, of silica and carburet of iron, so that the pure charcoal appears to constitute not more than one tenth of the primitive charcoal. It would consequently appear that since the discolouring power resides in the carbon, that of this purified carbon ought to be ten times more considerable than that of the crude charcoal; but this is by no means the case, for the discolouring power of the purified charcoal is only 1.5, that of the crude being 1. To account for this apparent anomaly, we must consider, as will be shewn in the next chapter, that when carbon blanches a solution, the colouring matter comes to be deposited on its surface; of consequence, the more surface the charcoal presents to the solution, the more easily will the discoloration occur. It is precisely the same effect, which is observed in dyeing fine or coarse wool; more colouring matter being required for the first, than for the last, in equal weights. Hence, when we treat bone black with muriatic acid, its discolouring force is found to be increased in the ratio of the foreign bodies which we abstract, and diminished in the ratio of the extent of surface which it loses; so that it is the relation of these two changes which determines the increase or diminution of its properties. None of the substances present in bone-black when used by itself, except the charcoal, discolours.

After establishing the difference which exists between the blanching property of different charcoals relative to the solution of indigo, it became interesting to know, if this relation would be preserved for the different colouring matters. M. Bussy made a test liquor with melasses; it was composed of one part of melasses, diluted with twenty of water, in which he tried the different charcoals marked in the following table. The quantities acted on were more considerable than those indicated in the table, but he reduced them to one gramme for the sake of comparison.

From this table we perceive; 1st. that the same quantity of the same charcoal discolours about ten times more of the indigo liquor, than of that of melasses; 2d. that the different charcoals preserve, in reference to these two substances, the same order in their blanching power, but that the proportion which exists between them, when they are tried by the melasses, is less than

when they are tried with the indigo. Thus in the case of indigo we find that the blanching power of blood-charcoal is to that of bone-black as fifty to one; and for the melasses, this proportion is twenty to one. To find out what might be the cause of this variation, he made the same trials on decoctions of cochineal, logwood, gum, *&c.*, and he observed that it always required more charcoal to blanch the same bulk of liquor, the more the relation between the discolouring power of the charcoals diminished; that is to say, if we suppose, that we take the like volume of all the test-liquors, that which will require least charcoal for its discoloration, is that which will establish the greatest difference between the discolouring force of the different charcoals.

Table of the comparative discolouring power of different Charcoals.

SPECIES OF CHARCOAL.	Weight.	Indigotest-liquor consumed.	Melasses liquor consumed.	Blanching power by the Indigo test.	Blanching power by the melasses test.
Blood calcined with potash . . .	1 gramme.	1 litre 6.	0.18	50	20
Blood calcined with chalk	id.	0.57	0.10	18	11
Blood calcined with phosphate of lime . .	id.	0.38	0.09	12	10
Gelatine calcined with potash . . .	id.	1.15	0.14	36	15.5
Albumen calcined with potash . . .	id.	1.08	0.14	34	15.5
Starch calcined with potash . . .	id.	0·34	0.08	10.6	8.8
Charcoal of acet. potash	id.	0.18	0.04	5.6	4.4
Charcoal from subcarb. soda by phosphorus .	id.	0.38	0.08	12	8.8
Calcined lamp black .	id.	0.128	0.03	4	3.3
——— with potash .	id.	0.55	0.09	15.2	10.6
Bone-black treated with muriatic acid and potash	id.	1.45	0.18	45	20
Bone-black treated with muriatic acid . .	id.	0.06	0.015	1.87	1.6
Vegetable or animal oil calcined with phosphate of lime . .	id.	0.064	0.017	2	1.9
Crude bone-black . .	id.	0.032	0.009	1	1

Since the Indigo liquor contains the thousandth part of its weight of Indigo; that is, one gramme per litre, we perceive that the numbers, which express the quantity of liquor blanched per litre, express the real quantity of Indigo fixed on the charcoal.

Chapter III. On the Mode of Action of Charcoal in discolouring.

It is generally thought that the charcoal acts on colouring matters, by decomposing them; and this opinion is founded on it having been observed, in treating different matters with charcoal, such as beer, mucilage, melasses, wine, *&c.*, that the discoloration was accompanied with a disengagement of gas. Presuming that this opinion was entirely erroneous, and that to the decomposition of the colouring matter, a disengagement of gas had been improperly ascribed, which must depend on some other cause, M. Bussy made the following experiments: He took 8 ounces of melasses, which he diluted with a pound and a half of distilled water, agitated it with carbonate of lime to neutralize the acids which might act on the charcoal; then filtered the liquid, and heated it to expel the gases which might be held in solution in the water.

On the other hand, he took 1 ounce of blood-charcoal very carefully washed; boiled it in 8 ounces of water, to expel all the air which might be adhering to it, and when he judged it to contain no more air, he added the melasses, so that the vessel which he employed, was perfectly full. The apparatus being thus arranged, and perfectly freed from air, he engaged the extremity of the tube, under a graduated tube filled with water; and he heated the vessel in the salt-water bath for two hours, at a temperature approaching to that of boiling water. No gas whatever was disengaged, and the liquor became completely colourless; the experiment succeeds equally well in the cold. He tried also cochineal, and several other colouring matters, and he obtained no evolution of gas. As to wine, we must take great care to employ a charcoal which has been previously treated with muriatic acid, for otherwise, the acids which it contains, decompose the carbonate of lime which is present in the charcoal, and gives rise to a disengagement of gas.

The colouring matters not being decomposed by the charcoal, as appears from this experiment, we conceive they must fix themselves in the charcoal, augment its weight proportionally to the quantity of liquor discoloured; and that, in some circumstances, we may re-produce the solution of the colouring matter.

To verify this conjecture, he took three species of charcoal, which he knew acted with a different intensity; these were: 1st, charcoal of blood; 2d, bone-black, purified; 3d, crude bone-black. These charcoals after having been washed with boiling water, to separate all their soluble ingredients, were dried at the water-bath, for the same space of time. He then took 5 grammes of each of these charcoals, which he heated separately with 20 grammes of melasses, diluted with 4 parts of water. This liquor was left in contact with each of the charcoals during the same time; at the end of which they were fil-

tered, and were found differently discoloured, according to the species of charcoal which had acted upon it. Each of these charcoals having been washed, and then dried at the water-bath, taking every precaution to place them in the same circumstances, it happened that

The blood-charcoal was increased in weight by 1.56 gr.
The purified bone-black 0.54
The crude bone-black 0.3

This increase of weight, which is, as we see, relative to the blanching power of each charcoal, indicates that the discoloration is effected by the combination of the colouring matter with the charcoal. The same effect is also remarked, when we employ indigo; but it is much less appreciable. We have reason, indeed, to be surprised at the considerable augmentation of weight which charcoal assumes in the discoloration of melasses; but it is probable, that this increase proceeds not only from the colouring matter, if there exists a particular one, but also from the mucilaginous matters, and others which are not in perfect solution. It is a fact, well known to refiners, that the waters of the charcoal washings which have served for clarification, are very mucilaginous. If, instead of taking cold water to wash this charcoal, boiling water be taken, slightly alkalized with potash, the water comes off coloured of the melasses tint, which further confirms the conclusion to be drawn from the augmentation of the weight of the charcoal, namely, that the colouring matter combines with the charcoal, without experiencing decomposition.

But this truth seems to be established in an incontestable manner, by an experiment which M. Bussy made on indigo.

Into a solution of indigo by sulphuric acid, made with one part of indigo, and 7 parts of acid, the whole diluted with 92 of water, he added a certain quantity of charcoal which completely discoloured it. The liquor thrown upon a filter, passed through with a slight tint of yellow, visible only in a great mass of the liquid. The filter was washed with a small quantity of cold water, to remove what of the solution remained adhering to the filter; this water passed colourless. He threw afterwards on the same filter, boiling water, containing a very small proportion of sub-carbonate of potash; instantly the liquor passed through of a blue colour, not of a feeble and uncertain tint, but with an intensity of shade, equal to that of the first solution. It is possible, by continuing the washing with very hot alkalized water, to remove from the charcoal almost the whole of the indigo which it contains.

If we take again this same washed charcoal, and put it in contact with the blue liquor of the washings, after rendering it slightly acid by the addition of a little sulphuric acid, it will discolour it anew, and the indigo fixed in the charcoal may be

once more carried off by the potash, so that we may make it pass in succession a considerable number of times, from the charcoal to the solution, and from the solution to the charcoal. It is, however, observed, that these successive solutions terminate in altering the indigo; and, at each new discoloration, the liquor retains a faint yellowish colour, which indicates the decomposition of a small quantity of indigo.

We may conclude from all that we have seen, that the discolouring property is inherent in charcoal; but that it becomes manifest only when the charcoal occurs in certain physical conditions, among which porosity and division hold the first rank; that no charcoal can discolour when it has been heated strongly enough to become hard and brilliant; that all, on the contrary, possess this property when they are sufficiently divided, not by a mechanical action, but by the interposition of some substance which opposes their aggregation; and that the superiority of animal charcoal, such as that of the blood and geltaine, proceeds especially from its great porosity.—*Jour. de Pharm.* June, 1822.

Analysis of the Bark of the Quassia Simarouba of Linnæus. By M. Morin.

1. A resinous matter;
2. A volatile oil having the odour of benzoin;
3. Acetate of potash;
4. An ammoniacal salt;
5. Malic acid, and traces of the Gallic;
6. Quassine;
7. Malate and oxalate of lime;
8. Some mineral salts, oxide of iron, and silica;
9. Ulmin and ligneous matter.

Analysis of the Smelt (Salmo Eperlanus of Linnæus). By M. Morin.

1. Albumen;
2. Mucus;
3. Osmazome;
4. Muriate of ammonia;
5. A gelatinous matter;
6. Muriate of potash.
7. Phosphates of potash, magnesia, iron, lime;
8. Carbonate of lime;
9. An oily matter;
10. Phosphorus;
11. Animal fibre.—*Jour. de Pharm.* Feb. 1822.

New Researches on the Composition of the antimoniated sulphuret of Silver, the red Silver of Andreasberg. By M. P. A. Bonsdorff.—The pounded ore was put into an apparatus, simi-

lar to that employed by Berzelius, for the analysis of the ores of nickel (see our last Number), and being heated with the flame of a spirit lamp, was subjected to a stream of hydrogen, evolved, from zinc and dilute sulphuric acid. In this way, the presence of oxide of antimony would be proved by the production of water, as he previously ascertained by a comparative experiment on an artificial compound. But no water was formed from the mineral.

The result of his analysis is:

				Oxygen.	Sulphur
Silver,	0.8865	58.94	which would	4.36	8.768
Antimony,	0.3436	22.84	require	4.19	8.423
Sulphur,	0.2498	16.61			
Earthy matter,	0.0045	0.30			
Loss,	1.3100	1.31			
		100.00			

The chemical constitution he represents by $2\,Sb\,S^3 + 3\,Ag\,S^3$, which would give the proportions,

Silver,	58.98
Antimony,	23.46
Sulphur,	17.56
	100.00

Klaproth, in his analysis, had ascribed the loss of weight to oxygen, and gave these proportions of the components,

Silver,	60
Antimony,	19
Sulphur,	17
Oxygen,	4
	100

By adding the weight allowed for the oxygen, to that of the antimony, Klaproth's analysis agrees very closely with M. Bonsdorff's. *Ann. de Chim. et de Phys.* xix. i.

Analysis of the mineral and thermal waters of Mont Dore.
By M. P. Berthier.

These waters are among the most celebrated of mineral springs. The Romans had collected them in a vast and sumptuous edifice. They are composed as follows:

	Anhydrous Salts.	Crystallized Salts.
Neutral carbonate of soda	0.0006330	0.0006930
Muriate of soda	0.0003804	0.0003804
Sulphate of soda	0.0000655	0.0001489
Carbonate of lime	0.0001600	0.0001600
Carbonate of magnesia	0.0000600	0.0000600
Silica	0.0002100	0.0002100
Oxide of iron	0.0000100	0.0000100
	0.0015189	0.0016623

This water is distinguished by the large quantity of silica

which it contains. The water deposits this silica in the subterraneous channels which it passes through, under the form of tubercular masses, often very large, and resembling flint. *Ann. de Chim. et de Phys.* xix. 25.

On the influence which water exercises on certain Animal Products. By M. Chevreul.

1. *Tendons.*—These, when dried, become much slenderer, lose their white colour, their satiny lustre, and extreme pliability. They acquire the semi-transparency of horn, a yellow colour, bordering slightly on red. Tendons of the elephant, preserved four years in the dry state, re-produce a fresh tendon, when kept some time immersed in water.

Table of experiments on Tendons.		Water absorbed by 100 parts of the opposite bodies, after immersion of 12 to 24 hours	
100 parts of a large tendon of the elephant, are reduced by drying in the air, to	51.56		
In vacuo, to	50.00	8 days at least	147. 0
100 parts of a slender tendon of the same—in air, to	46.91	12 to 24 hours	130. 3
In vacuo	43.36	8 days at least	147 68
100 parts of a large ox tendon are reduced in air, to	52.96	12 to 24 hours	100.34
In vacuo, to	49.61	8 days at least	146.58
100 of slender ox-tendon in air, to	44.15	12 to 24 hours	132. 0
In vacuo	42.34	8 days at least	148. 0
100 of slender human tendon become in air	43.13	12 to 24 hours	147.87
In vacuo	37.98	5 days	271.79
2. *Yellow elastic tissue.*— 100 parts fresh from the elephant are reduced in air, to	52.57	24 hours	99. 0
In vacuo, to	50. 5	12 days	147. 0
100 parts fresh from the ox—in air	52. 8	24 hours	99. 4
In vacuo to	49. 8	12 days	148. 0

3. *Cartilage of the Ear.*—100 parts of that of a man of forty years, after it was steeped in water, were reduced in the air to 33.5, and *in vacuo*, to 30.64. These 30.64 parts had absorbed at the end of 24 hours, 66.14 of water, and at the end of four days, 69.36, exactly what they had lost by desiccation.

4. *Cartilaginous ligaments.*—100 parts of the cartilaginous ligament of the knee of a woman, thirty years of age, after having been steeped in water, were reduced, in air to 26.41, and *in vacuo* to 23.2.

5. *Of Fibrina.*—100 parts from the arterial blood of a cow were reduced in the air to 21.1, and *in vacuo* to 19.35. 100 parts from the venous blood of a cow, became in air 22.7; *in vacuo* 21.05.

6. *Cornea.*—Of opaque cornea, dried *in vacuo*, 100 parts became, after an immersion in water of

24 hours, 268.18 4 days, 461.28.

7. *Albumen of the Egg.*—100 parts of liquid albumen, after being coagulated, by exposure to air, became 15 parts; and *in vacuo* 13.65. The dry substance was colourless, semi-transparent, and it recovered the properties of boiled white of egg in absorbing water, which, however, it took up only to 68 parts after four days' immersion, instead of the 86.33, which it had lost. White of egg uncoagulated, dries without ceasing to be transparent. It loses in air 85 *per cent.*, and *in vacuo* 86.15. On immersion in water it resumes its original viscid appearance.—*Ann. de Chim. et de Phys.*, xix. 32.

Cure of a Palsy by a stroke of Lightning.—Mr. Samuel Leffers, of the county of Carteret in North Carolina, had been attacked with a palsy in the face, and particularly in the eyes. While he was walking in his chamber, a thunder-stroke threw him down senseless. At the end of 20 minutes he came to himself; but he did not recover the entire use of his limbs till the evening. Next day he found himself perfectly recovered; and he could now write without the use of spectacles. The palsy did not return.

Analysis of the Table-spar of Pargas, the Wollastonite of Haüy. By M. P. A. de Bonsdorff.—The colour of this table-spar is a white, more or less pure; it is translucid on the edges; its lustre is vitreous and moderate; it is semi-hard, scratching glass with difficulty; under the hammer it splits into fine flexible threads. Exposed to the blow-pipe, it melts on the edges at a strong heat, affording a translucent glass, colourless and brilliant. With borax, and the double phosphate of soda and ammonia, it melts into a transparent glass; and gives with soda an opaque globule. With solution of cobalt, it presents a blue colour, a property which does not seem to agree with the nature of tremolite, which treated with the same solution, exhibits usually a flesh colour. Its analysis afforded,

Silica	52.58
Lime	44.45
Magnesia	6.68
Protoxide of iron	1.13
Alumina, a trace	
Volatile matter .	0.99
	99.93

M. Bonsdorff considers it as a bisilicate of lime, or $C S^2$.

Analysis of the Meteoric Stone of Juvenas, by M. Laugier.—Four analyses were made; the first by means of acids; the second with potash; the third with nitric acid, to determine the proportion of sulphur; and the fourth by nitrate of barytes, with the view of ascertaining the proportion of potash which M. Vauquelin had detected in this stone. These different ana-

lyses all concurred very nearly as to the proportions of the constituents, which are,

Silica	40
Oxide of iron	23.5
Oxide of manganese	6.5
Alumina	10.4
Lime	9.2
Chromium	1.0
Magnesia	0.8
Sulphur	0.5
Potash	0.2
Copper	0.1
Indispensable loss	3.0
Loss from unknown causes	4.8
	100.0

This remarkable loss, instead of the usual increase of weight, in the analysis of meteoric stones, by the oxidation of the metals, has led M. Laugier to infer that the iron and manganese are here in the state of oxides ; and there is no particle of this meteorolite, when pulverized, which is attractible by the magnet. This aërolite seems identical in its composition with the aërolite of Jonzac. The complete absence of nickel, the almost total disappearance of the sulphur and the magnesia, which are replaced by an abundant quantity of lime and alumina, establish between these stones, and the aërolites previously known, very marked differences.

A third example of this peculiar composition is presented in an aërolite, which fell in the neighbourhood of the village of Lontola, in the government of Wibourg, in Finland. M. Nordenskiold, mining engineer at Abo, a pupil of Berzelius, found no nickel in it, and hardly any metallic particles attractible by the magnet.

M. Laugier here concludes with still more certainty than he did in 1820, that chromium is the most constant character of aërolites.

The most numerous and best known meteoric stones have more solidity than these new ones. Globules of iron are found in them, which resist the pestle, and are attracted by the magnet. The latter are friable, easy to pulverize, presenting no obstacles in the mortar, and contain no globules of iron. Their small cohesive force seems to arise from the interposition of different foreign bodies, such as felspar or amphigène. They are, however, less homogeneous in their structure, than those first known. *Ann. de Ch. et de Phys.* xix. 264.

[The late arrival of the Foreign Journals has prevented us from arranging their contents in a methodical order.]

Art. XVIII. MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

§ Astronomy, the Arts, &c.

1. *Improved Signal for Trigonometrical Measurements.*—A new kind of signal has been used by M. Gauss of Gottingen, in a trigonometrical measurement undertaken in Hanover, which appears to possess many advantages. It consists of reflected solar light: that astronomer having remarked that the light reflected by a small plane mirror, was sufficiently intense to be observed at greater distances than those of the sides of his greatest triangles, had a couple of instruments made which he called Heliotropes, and which, though simpler in their construction than the Heliostat of S'Gravesande, were, like it, intended to reflect the sun's rays in a constant direction. Whilst these instruments were preparing, M. Gauss made use of Hadley's sextant, which, for this purpose, was mounted on a solid foot in the following manner: The plane of the instrument being inclined to the proper degree, the angle between the sun and the terrestrial object to which the sun's rays are to be reflected by the moveable or great mirror of the instrument is to be observed. Then, without altering the apparatus otherwise, the arm which carries the mirror is to be moved, until the index is at double the angle observed, when the sun's rays will be reflected on the spot desired, so that from that point the image of the sun may be seen in the mirror like a star.

The same result may be obtained by previously fixing a third mirror above the moveable mirror, on the same arm, and perpendicular like it to the plane of the instrument; but which makes, with the plane of the great mirror, an angle equal to the complement of 90 degrees of the angle formed by the visual ray with the plane of the third mirror. When, with a sextant thus prepared and fixed on a foot, the distance of the object from the centre of the sun is observed, this third mirror reflects the sun's rays at the same moment on the object itself, and an observer being there, the signal will be the light reflected from the sextant. It is easy, by a little practice, to give that motion to the mirror which is necessary to throw the sun's rays for some time on the same object; for, in consequence of the imperfection of the mirror, the field over which the rays are reflected is large enough to compensate for any little irregularity in the movement.

M. Gauss has found that mirrors, two inches by an inch and a quarter, are quite large enough for these purposes: In some experiments, made with a view of estimating the distance to which

these signals would be visible, a heliotrope and a sextant were placed two geographical miles from each other, the luminous points reflected by those mirrors could be seen by the naked eye, and when viewed through the telescope of the theodolite were too brilliant to give exact points; but when, in place of the sun's light that from a bright cloud was reflected by them, they gave an excellent mark.

At a distance of five miles, the points were still visible, like stars, by the naked eye, and they could be seen through the telescope of the instrument, even in heavy weather, when the great signal itself could not be distinguished. At last the distance was increased to 11 or 12 geographical miles, the stations being at Inselberg and Hohenhagen, and the operators M. M. Gauss and Enke. The light was reflected at intervals by the sextant from Inselberg to Hohenhagen, whilst the light was constantly reflected from the latter to the former place by the heliotrope; these experiments continued 10 days in various circumstances with great success. Each observer reciprocally saw the points at the other station, whilst frequently the mountains on which they were placed could scarcely be seen by the telescope. More than once the light of the heliotrope pierced through mists and even showers of rain.

In general the angles observed by these signals were in greater accordance with each other, than when the ordinary signals have been used.

Baron de Zach proposes a simplification of the reflecting apparatus. He supposes a polyhedral reflector, similar to those sometimes used by bird-catchers, to rotate on an axis passing through the number of its facets, there will then continually be a facet reflecting the light in the proper direction; and farther, such a signal would be visible in all directions. In a trial of this process made in the small way, a piece of rock crystal, cut with many facets, was made to rotate rapidly in the sunshine, it was seen distinctly from all situations at the distance of 2,000 toises.—*Bib. Univ.* xviii. 151.

2. *New Observatories.*—Three new observatories have been established in countries the most remotely situated from each other:—at Nikolajen, on the borders of the Black Sea; at the Cape of Good Hope; and in New Holland.

3. *Astronomical Prize.*—The medal founded by M. de Lalande, to be given annually to the person who, in France, or elsewhere, has made the most interesting observation in astronomy, or rather the mémoire most useful to its progress, has not been adjudged in 1822. So that the prize for 1823 will be doubled, and will consist of a gold medal, value 1,270 francs. It will be adjudged in March 1823.

4. *Cleansing of Orchard Trees by Lime.*—The use of lime has been highly recommended in the dressing of old moss-eaten orchard trees. Some fresh-made lime being slaked with water, and some old worn-out apple-trees well dressed with it with a brush, the result was that the insects and moss were destroyed, the outer rind fell off, and a new, smooth, clear, healthy one formed: the trees, although twenty years old, assuming a most healthy appearance.

5. *Purification of Oil.*—A method of purifying common fish oil, and rendering it equal to the best sperm oil, by the use of animal charcoal, is described as having been discovered in Denmark. The description is very incomplete, but mentions that beef bones which have been boiled, are made into animal charcoal in a peculiar way. The charcoal is mixed with the oil, and repeatedly agitated for two months, after which it is filtered through several strata of charcoal, and used as soon as made. The quantity of gas evolved by the bones in the operation is considerable, and is used for lighting the manufactory and adjacent buildings. The residuum is mixed with clay for fuel. The loss in this process is estimated at 15 per cent., and the gain is equal to 40 per cent., leaving a balance in favour of the discovery of 25 per cent.

The peculiar method of making the charcoal, probably consists in not heating the bones too much. It is well known by the animal charcoal makers in London, that if the temperature be raised too high, the charcoal is worth nothing.

6. *Oil Gas.*—Mr. Wilson proposes obtaining this gas in countries where the oil is chiefly vegetable, by introducing the seeds themselves into the retorts, much in the manner that coals are used here. Besides saving the expense of preparing the oil, it is supposed that the charcoal left may be useful and valuable. We, however, greatly doubt the plausibility of this proposal.

7. *Purification of the Water of the Seine at Paris.*—There is an establishment in this city (Paris) for purifying for domestic use the water of the Seine, which gives constant employment to upwards of 200 persons. The water is pumped into vessels about 20 feet deep, and as many in width, where it reposes 12 hours. The clear water is then raised into another vessel, whence it flows into long and shallow cisterns, on the sides of which a great number of sponge filters are placed, and the sponges are renewed every hour. From the sponge filters it finds its way into square shallow cisterns, each of which has at the bottom a bed of clean Fontainebleau sand, then a bed of pounded charcoal, then another bed of clean sand, and lastly, at top, a bed of coarse river sand, (these last-mentioned filters are renewed every six hours); and

this is the last operation previous to its distribution.—*Tech. Rep.* i. 316. We apprehend this process would not suit the London taste.

8. *Preservation of Steel Goods.*—Mr. Aikin recommends a thin coating of caoutchouc as an excellent preservative of iron and steel articles from the action of the air and moisture; its inalterability, consistence when heated, adhesion to iron and steel, and facility of removal, render it an admirable substance for this purpose.

The caoutchouc is to be melted in a close vessel, that it may not inflame. It will require nearly the temperature of fusing lead, and must be stirred with a horizontal agitator rising through the vessel, to prevent burning. Plates of iron and steel, partly covered with this composition, gave full proof in the laboratory of the protection afforded by the caoutchouc film to the metal.

Mr. Parkins, to whom Mr. Aikin communicated this process, has made much use of it in his blocks, plates, dies, &c. He mixes some oil of turpentine with the caoutchouc, which renders it easily applicable, and leaves the substance, when dry, as a firm varnish impermeable to moisture. This, when required, is easily removed by a soft brush dipped in warm oil of turpentine.—*Tech. Reports*, i. 55.

9. *Preservation of Eggs.*—Eggs may be preserved, according to M. Cadet, a great length of time in lime-water containing excess of lime. An excellent mode of preserving them also, is to place them for about 20 seconds in barley-water; then remove, dry, and put them by. Eggs may be well salted throughout, if laid in brine for 8 or 10 days, and may then be preserved a great length of time.

10. *Fusible Metal, and its application.*—A combination of three parts of lead, with two of tin and five of bismuth, forms an alloy which melts at the temperature of 197° F. In making casts with this and similar alloys, it is important to use the metal at a temperature as low as possible; as if, but a few degrees elevated, the water which adheres to the things from which casts are to be taken, forms vapour, and produces bubbles. Mr. Varley allows the fused metal to cool in a tea-cup until just ready to set at the edges, and then pours it into the moulds, procuring in this way beautiful casts from moulds of wood, or of other similar substances. When taking impressions from gems, seals, &c., the fused alloy should be placed on paper or pasteboard, and stirred about till it becomes pasty from cooling; at which moment the gem, die, or seal, should be suddenly stamped on it, and a very sharp impression will then be obtained.

Mr. Gill has applied the fusible alloy to the formation of metallic pencils. The metal is cast into a proper form in a piece of paper over a candle. A sheet of drawing-paper is prepared by having a little prepared hartshorn, (i. e., finely divided calcined

bones), well rubbed into its surface by means of a coil of list, or woollen cloth, the excess being gently wiped off by a cloth.

11. *Composition for Moulds.*—Moulds, formed from a composition of sulphur and iron scales, have been successfully employed in America in place of those usually made of brass, enduring the necessary pressure without injury. They are formed by dissolving the pulverised iron scales from a smith's forge in a melted sulphur: the proper proportions may be readily found by experiment. The compound is easily fusible, and takes very sharp and accurate casts from the originals.—*Tech. Rep.* i. 446.

12. *Cement.*—The following is a very excellent cement for the use of turners and artisans in general. The receipt is due to Mr. S. Varley: 16 parts of whiting are to be finely powdered and heated to redness, to drive off all the water. When cold, it is to be mixed with 16 parts of black resin, and 1 part of bees-wax; the latter having been previously melted together, and the whole stirred till of an uniform consistence.—*Tech. Rep.* i. 416.

13. *Hyalograph.*—M. de Clinchamp, teacher of the youth intended for the marine service at Toulon, in France, has invented an instrument called a Hyalograph, which takes off, with extreme correctness, the appearances of natural objects. A particular sort of ink serves to impress on paper designs taken on the glass of the instrument; and as the first impression cannot be erased, many proofs may be taken off, by inking it over again. The hyalograph serves also for many mathematical applications. The works executed with hyalographic ink have the appearance of lithographic designs.

14. *Black Lead Pencils.*—Hard black lead drawing pencils are made by Mr. Varley and Mr. Banks, by melting together fine Cumberland black lead in powder and shell lac. This compound is repeatedly powdered and re-melted, until of uniform composition; is then sawn into slips, and mounted as usual. Pencils thus made are uniform, and of great strength, and there is no waste of material.—*Tech. Rep.* i. 286.

ii. Chemical Science.

§ 1. *Chemistry.*

1. *On the Chemical Phenomena of thin Plates.*—Sig. A. Fusinieri has inserted an account of a long series of experiments on the chemical action which takes place in very thin layers of various substances. The fixed and volatile oils, alcohol, ether, balsams, *&c.* were placed on glass, water, and mercury, in the air, *in vacuo*, in different gases, and in different positions, and the phenomena, carefully observed. These are detailed in the *Giornale di Fisica*, iv. pp. 133, 209, 287, and are to be continued; but the following are the general principles deduced from the three first parts of the mémoir.

1. All liquid substances reduced to uniform laminæ, so thin as to decompose light by reflection and transmission, have the power of throwing off their own substance in the direction of the angle of the wedge parallel to the plane, which passes by the angle perpendicularly to the thickness.

2. This expulsive force is in the same substance always less as the wedge is more acute, and greater as the wedge is less acute, within certain limits not yet determined.

3. From this force proceeds the spontaneous expansion of liquid masses reduced in some parts to their cuneiform plates, and continued in that direction (1), if the movement be not impeded by external obstacles. In this way drops which form themselves into spherical segments on horizontal surfaces that have no attraction for the drops, and admit of free motion, expand in all directions.

4. If the expansion is impeded, not by external obstacles placed in the line of its direction, but by some other force which obliges the wedge-shaped lamina to preserve its form, then, by the action of the same force, the substance is successively withdrawn, and extended in the same direction.

5. When the obstacles to the motion are opposed to the angle of the wedge, then the force re-acts in a direction normal to the resistance, and therefore nearest to the primitive direction, producing expansion in the new direction.

6. This force of expansion is smallest in water and aqueous solutions, much greater in all combustible liquids, and among these greater with the odorous than the inodorous substances. It is greatest of all in the odorous and volatile substances. In the sulphuric and nitric acid it exists in a degree equal to that of the volatile combustible liquids.

7. The vapours which arise from the laminæ of volatile liquid combustibles, if retained in contact with their respective substances, impede the expansion of re-action in the direction normal to the plane of the lamina (5).

8. The force of expansion spoken of is essentially distinct from that by which bodies tend to form vapour: for the degree of this force has no relation to the degree of evaporability, and sometimes has a contrary ratio.

9. This force has, in common with the forces of electricity and magnetism, the power of increasing its proper action in the direction of the angles, of rendering it more intense, and of determining a current of matter in that direction.

10. Finally, The effects of this force are not limited to mere expansion, but consists also in chemical changes, decomposition taking place, and re-compositions being produced.

2. *Effect of Heat on the colour of the Ruby.*—In subjecting rubies to high degrees of heat, Dr. Brewster observed a very singular effect produced during their cooling. At a high temperature the red ruby becomes green; as the cooling advances, this green tint gradually fades and becomes brown, and the redness of this brown tint gradually increases till the mineral has recovered its primitive brilliant red colour. A green ruby suffered no change from heat, and a bluish green sapphire became much paler at a high heat, but resumed its original colour by cooling. *Edin. Journ.* vi. 379.

3. *Pyrometer.*—Mr. Sivright, proposes to remove the objections to Mr. Wedgewood's pyrometer, in consequence of the irregular contraction of the clay-pieces used, by substituting for them pieces of agalmatolite, or figure-stone of China. In making experiments on this substance, he has found it capable of contracting in its dimensions considerably, by heat, without any other change, and he considers that its use will be advantageous.

4. *Source of Cadmium.*—Mr. W. Herapath states, that he has obtained cadmium in abundance from the zinc works near Bristol. Zinc is obtained by putting calamine with small coal into a crucible, which being closed at top, has a tube proceeding through its bottom, into a vault below; beneath the tube is a vessel of water, and a short tube is at hand to connect at a proper time with the long one, so as almost to reach the water. The workmen do not connect until the "*brown blaze*" is over, and the "*blue blaze*" begun. This brown flame is owing to cadmium, the oxide of which attaches itself to the roof of the vault, in greatest quantity just over the orifice. It is mixed with soot, sulphuret of cadmium, and oxide of zinc. Some portions contain from twelve to twenty per cent. of cadmium.

The metal is obtained by dissolving this substance in muriatic acid, filtering, evaporating to dryness, re-dissolving and filtering, and precipitating by a plate of zinc. The cadmium thrown down is to be mixed with a little lamp-black or wax, put into a black

or green glass tube, and placed in the red heat of a common fire, until the cadmium has sublimed into the cool part of the tube; then the residuum is to be shaken out, which is easily done without loss of cadmium: a little wax introduced into the tube, and a gentle heat applied, the metal melts, and by agitation forms a button.

Mr. Herapath thinks that the zinc smelter, if he were to put up his tube earlier, and collect the first few pounds of metal separate, would be able to collect abundance of cadmium, and afford it at a cheap rate for the purposes of the arts.—*Annals of Phil.* iii. 435.

5. *Crystallised gold.*—When a solution of gold in ether is left for a considerable length of time, the gold is gradually reduced, and deposited in the metallic form, and crystallized.

6. *Stanniuretted Hydrogen.*—Professor Kastner has observed, that when tin is treated with moderately strong muriatic acid, the hydrogen gas developed is combined with tin. It has a very particular and penetrating odour: when compressed into water, a considerable quantity is dissolved; when burnt it produces a blue light, and gives off white oxide of tin in fumes. When introduced into a very weak solution of gold, purple of Cassius is instantly formed; and, applied in this way, the gas is a highly-sensible re-agent for the minutest portion of the noble metal. Bismuth, when similarly treated, forms a similar substance. Both these gases considerably resemble telluretted hydrogen.

It may be observed, that, when treated in the same way, zinc also produces a gas, containing large quantities of the metal when recent.

7. *Deutoxide of Copper.*—M. Berzelius says, "the deutoxide of copper attracts the humidity of the atmosphere very rapidly: it is reduced so rapidly in hydrogen gas, that if a piece be highly heated, but not red, and plunged into a bottle of the gas, the oxide takes fire and is reduced, and water trickles down the phial. According to the weight lost in this reduction, it appears to be composed of

Copper	.79825	100
Oxygen	.20175	25.272
	1.00000	

Ann. de Chim. xvii. 26.

8. *Preparation of Kermes Mineral.*—M. Hensmans recommends, that in the preparation of this substance, the sulphuret of antimony, should be fused with caustic potash, and not with a carbonate: for then, when a solution is made, and a current of carbonic acid

passed through it, abundance of kermes falls down; and on the addition of diluted sulphuric acid to the solution so treated, a second portion of the golden sulphuret is obtained: both these preparations have a fine and beautiful appearance.

9. *Jodo-cyanuret of Potassium and Mercury.* When solutions of prussiate of mercury, and hydriodate of potash, are mixed, abundance of crystals fall down, which, when collected, washed, and re-crystallized, appear in large thin brilliant plates unalterable in the air, without smell, but in solution having the odour of bitter almonds; soluble in 16 times their weight of cold water, and in much less when hot; soluble in 96 parts of alcohol. Their composition has not been determined, but 24 parts gave, with hydro-sulphuret of soda, 13 parts of sulphuret of mercury.—CAILLOT. *An. de Chim.* xix. p. 280. *(See our Foreign Science.)*

10. *Hydriodide of Carbon.* In the Philosophical Transactions, for 1821, I have described a compound of chlorine and olefiant gas, but had not at that time the means of ascertaining its composition. Since then, I have obtained it in greater quantity, and analyzed it. Four grains were passed in vapour over heated copper, in a green glass tube; iodide of copper was formed, and pure olefiant gas evolved, which amounted to 1.37 cubic inches. As 100 c. i. of olefiant gas weigh about 30.15 grs., so 1.37 c. i. will weigh 0.413 of gr. Now 4 grains minus 0.413 leaves 3.587 iodine, and 3.587 : 0.413 : : 117.75 : 13.55 nearly. Now 13.55 is so nearly the number of 2 proportions of olefiant gas, that the substance may be considered as composed of

1 proportion of iodine	117.75
2 proportions of olefiant gas . .	13. 4

and is therefore analogous in its constitution to the compound of chlorine and olefiant gas, sometimes called chloric ether. *M. F.*

11. *Formic Acid, and Formate of Lead.* The following estimations of the composition of these bodies is given by Dr. Gobel from careful experiments:

Formate of lead		1 atom oxide of lead . .	107.5
		1 —— formic acid . .	34.9
		1 —— water	8.45
Formic acid			
Carbon . . .	11.35,	or 2 atoms oxide carbon . .	26.4
Hydrogen . .	1.06,	1 —— water	8.45
Oxygen . . .	22.43		
	34.84		34.85

Annals Phil. iii. 463.

12. *Dilated Caoutchouc Bottles.*—Mr. Forster has shewn, that caoutchouc bottles may be expanded, until so thin and large as to answer many purposes of bladders, in philosophy and the arts. His process is to condense air into the caoutchouc which gradually forms the appearance of a blister at the bottom of the bottle, and which, extending, at last includes the whole. In this way the bottles have been made 6 and 7 inches in diameter; and by care, Mr. F. thinks may be made thin enough for balloons. When rubbed on warmed paper, these globes become very strongly electrical.—*Phil. Mag.* lix. 263.

13. *Prize Question.*—The Royal Academy of Sciences has proposed the following prize subject for 1824.

1. To determine, by multiplied experiments, the density acquired by liquids, and particularly by mercury, water, alcohol, and ether, under pressure equal to the weight of many atmospheres.

2. To measure the effects of the heat produced by these compressions.

The prize is a medal of gold, value 3000 francs. The memoirs to be sent in before January 1, 1824.

§ 2. Electricity, Magnetism.

14. *Pyro-Electricity of Minerals.*—M. L'Abbé Haüy has remarked, with regard to the electricity produced in certain crystals by an alteration of temperature, that it is of two kinds. The accidental circumstances which led to the discovery took place whilst he was examining some crystals of the oxide of zinc, from Limbourg, near Aix-la-Chapelle, and fragments of the acicular variety of the same mineral from Brisgau. Having placed a piece of one of these substances in a very cold window for a few moments, it was found on examination to be electrical. Its poles were ascertained, and the mineral then placed in a milder temperature, when the electricity soon became nul; but being approached to a fire-place, the power was greatly renewed, but the poles were inverted.

These results have been verified by M. Haüy on other crystals, especially those of the tourmaline. In taking them for examples, he has endeavoured to bring under one point of view all that passes with respect to them in the interval comprised between the limits of temperature, beyond which the electric action disappears without return. He has given the name of *ordinary electricity* to that produced by heat, and *extraordinary electricity* to that produced by cold. If, therefore, commencing at the point where the excess of heat destroys in the tourmaline the effects of the ordinary electricity, that mineral be left to cool it will soon give signs of ordinary electricity. The action of

the poles at first feebly augments to a certain degree, beyond which it gradually diminishes, and at last disappears. With a temperature a little lower, however, the extraordinary electricity appears, and the poles resume their power, but in an inverted order, so that the pole at first positively electrified becomes negative, and the negative pole becomes positive.

These results are detailed at large in M. l'Abbé Haüy's *Traité de Physique.* We have taken these from the *Annales de Mines.* vi. p. 608.

15. *Electricity of the Atmosphere.*—M. Bourdet, an ex-captain in the French service, has described, in a letter, a very singular electrical phenomenon which he witnessed in Poland, Dec. 24, 1806. The weather, according to the Poles, had never been milder at that season of the year, no snow had been seen, nor had the usual cold weather of the north, which generally sets in early in that country, then commenced. Rains and storms, however, were frequent. "I was," says M. Bourdet, "with the advanced guard of light cavalry; the commander gave me an order to halt in the rear and see that my guns were disembarrassed, and then to rejoin, as quickly as possible, the light brigade. In spite of the efforts of my men, the guns were not cleared from the marshy ground in which they were entangled, without great labour. We were advancing across the field about nine o'clock in the evening, when a strong gust of wind suddenly arose, (the sun had shone brightly during the day,) and in a few minutes after, the night became so dark, that we could not see the heads of our horses. The wind blew so violently, that the horses stopped. At that moment the extremity of the hair on their ears became luminous, as well as all the longer hairs on their bodies, except the locks on their manes and tails. All the metallic extremities of their harness, and all the metallic sharp points of the carriages of our guns, were studded with luminous points, so that one might have supposed, had it been spring, that a swarm of glow-worms had covered our horses and guns. Our quarter-master observed that the points of my mustachios were luminous. The same phenomenon was seen on some of the cannoneers, but none of us had our eye-lashes or hair rendered luminous. These lights remained as long as the gust of wind lasted, namely, for three or four minutes. Their colour was a soft violet, and they terminated in a bright white. The horses held their heads high, their ears were erect and moving, their nostrils open and respiring, their manes and tails erected, their fore legs thrown forward, and their hind ones back. Their attitude, in general, was that of animals seized with terror. During the time the wind blew, they remained at full stop, and when feeling the spur, some stood stock still, and others kicked, as if they had been reluctant to advance. When the

wind ceased, the lights disappeared, and a deluge of rain, mingled with hail, fell. But though the obscurity continued, our horses moved on, shaking themselves at times, panting forcibly, and neighing; but they continued on their march. On arriving at the advanced post, I mentioned to my comrades the phenomenon we had witnessed, and though they had been only three leagues from us, they had felt no wind, but experienced much rain. The wind we encountered had an opposite direction to the rain."

16. *Electrometer for Minerals.*—M. Razoumansky describes an electrometer, which is far more sensible than M. l'Abbé Haüy's, though the only difference is that the needle he uses on the pivot is of steel, and is magnetized. By trials with gems and stones, the instrument is said to have been affected more readily than the Abbé's, and without requiring that attention to temperature which M. Haüy directs. M. Razoumansky attributes its excellence to its being magnetic; but it is by no means evident how the magnetism should improve it, since the electricity in the state in which it exists on the rubbed or otherwise treated gems, would have no influence on that power.—*Journal de Phys.*, xciii. p. 398.

17. *Remarkable Luminous Phenomenon.*—M. Doe, being surprised by the night, whilst in the forest near Boulancourt, in the neighbourhood of Brienne, department de l'Aube, was witness to a very brilliant luminous phenomenon which took place in a neighbouring marsh. The appearance began about two o'clock in the morning, apparently at one of the openings of the wood on the western side, whilst the sky was serene, the stars brilliant, and the air calm and temperate. The fire burnt quietly, without jets or undulations, in the form of a quadrangular pyramid. The colour of the flame was a pale red, verging on white, and the appearance of the whole was compared by M. Doe to sun-set, when it takes place behind a red cloud, girt by a dark zone. On proceeding to the place where this phenomenon had its rise, it was found to be a marsh about half a league in extent, traversed by ditches, which furnished the phosphoric matter of the flame. The greatest height of the luminous meteor was ten or twelve feet; there was no heat, but the light was bright enough to permit of reading by it. In about half an hour the effect was considerably diminished, and at the expiration of an hour it had entirely ceased.—*Journal de Phys.*, xciii., 236.

18. *Terrestrial Magnetism.*—The following are extracts from a letter written to M. Rumker by Professor Hanstein, containing a short account of his own discoveries.

" I observed here at Christiana, in the months of November

and December 1819, and in March, April, and May, 1820, seven or eight times every day, the time of 300 oscillations, (performed by a magnetic steel cylinder, suspended by a very fine silk thread,) by which I have found :

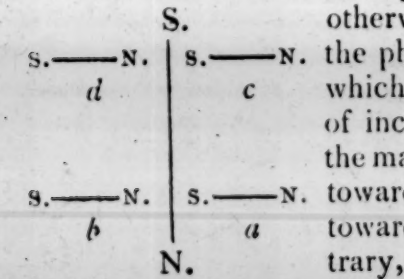
First, That the magnetic intensity of the earth is subject to a diurnal variation, so that it decreases from the first hours of the morning till about ten or eleven, when it arrives at its minimum ; from that time it goes on increasing till four in the afternoon, and in the latter months till six or seven in the evening. This force afterwards decreases anew during the night, and about three in the morning reaches its maximum, when it again returns, by little and little, to its minimum about ten or eleven in the morning, and so on continually.

Second, That whenever the moon passes the equator, the magnetic intensity is considerably weaker in the two or three following days.

Third, That the magnetic intensity is still more reduced during the appearance of an aurora borealis, and is so much the weaker as this meteor is extensive and powerful. The common intensity returns only by degrees, and 24 hours afterwards.

Fourth, That the magnetic intensity appears to have a very considerable annual variation, being stronger in the winter months than in the summer months.

" My second magnetic discovery is the following:—I have found that every vertical body S. N. whatever it be, and of whatever kind of matter has a north pole at bottom, and a south pole at top, as in all vertical bars of iron, for otherwise it would be impossible to explain the phenomena which I have observed, and which I have determined by a great number of incontestible experiments, namely, that the magnetic cylinder oscillates more quickly towards the north in *a*, and more slowly towards the south in *b*. And, on the contrary, it oscillates more slowly towards the north in *c*, and faster towards the south in *d*. I have found this law constantly confirmed by my experiments near the walls and partitions of houses, whether of wood or stone, and even near large trees in the garden. This action must necessarily exert its influence on the direction of the compass-needle on ship-board. The whole mass of wood in a ship has in this way a magnetical axis, and the observed variation of the compass ought rather to be attributed to this influence than to that of iron, guns, and ballast carried by the vessel. Hence it results that all observations on the mag-



netic intensities made within doors are uncertain.—*Zach's Correspondence.*

19. *Intensity of the Magnetic Force in different parts of the World.*—The following table is the result of Professor Hanstein's laborious observations.

Places.	Diff.	Intensities.
Peru	0° 0′	1.0000
Mexico	42 10	1.3155
Paris	68 38	1.3482
London	70 33	1.4142
Christiana	72 30	1.4959
Arendahl	72 45	1.4756
Brassa	74 21	1.4941
Hare Island	82 49	1.6939
Davis' Straits	83 08	1.6900
Baffin's Bay	84 25	1.6685

Zach's Correspondence.

20. *Magnetism affected by Earthquakes.*—M. Arago has transmitted to the French Academy of Sciences, an account of an observation he had made, which proves that the recent earthquake, the shocks of which were felt at Lyons and its neighbourhood, also extended its action to Paris. M. Arago has an observatory at Paris, for the purpose of observing the variations of the magnetic needle. On the 19th February, the needle remained perfectly steady until half-past eight o'clock; at a quarter before nine, it became agitated in a very extraordinary manner, with an oscillatory motion in the direction of its length. On observing this truly singular phenomenon, M. Arago was of opinion that it was occasioned by an earthquake.

At the same day and hour, M. Biot remarked an oscillating movement produced by the same earthquake, at his own residence in the College de France.

iii. Natural History.

§ 1. *Mineralogy, Geology, Meteorology.*

1. *Analysis of Yellow Copper Ore, by Mr. Phillips.*—Mr. Phillips has lately analyzed the different varieties of yellow copper ore, and has arrived at the conclusion that they are all of similar composition. His process was, after pulverizing the ore, to heat it in nitro-muriatic acid until the sulphur was acidified, filter, precipitate by nitrate of barytes, and collect the sulphate; then separating the excess of barytes from the solution by sulphate of

soda, to add to the clear solution ammonia, and collect the oxide of iron; the ammoniacal solution was then heated with potash, so as to drive off the ammonia, and the copper thrown down collected. By the appropriate tests, lead and arsenic were ascertained in the ore, if present.

Guineveau has analysed the amorphous yellow copper ore. The varieties analysed by Mr. Phillips are the crystallised and the mamellated. The composition of the three kinds are as follows;

	Amorphous.	Crystalysed.	Mamellated.
Sulphur	36.36	35.16	34.46
Iron	33.00	32.20	30.80
Copper	30.64	30.00	31.20
Earthy matter	0.0	0.50	1.10
Lead arsenic & loss	0.0	2.14	2.44
	100.	100.	100.

The mamellated variety contained no lead.

A compound of two atoms proto-sulphuret of iron, and four of per-sulphuret of copper, would consist of

4 atoms sulphur	64, or	34.78
2 —— iron	56, --	30.44
1 —— copper	64, --	34.78
		100

If, Mr. Phillips observes, the lead arsenic and earthy matter be neglected, and its place be supplied by copper, then the composition of the crystalline variety will approach nearly to, and that of the mamellated variety be almost exactly, that of the atomic composition.—*Ann. Phil.* iii., 301.

2. *Native Carbonates of Manganese.*—The carbonate of manganese of Nagiac contains, according to M. Berthier,

Quartz	.210
Protoxide of manganese	.443
Lime	.043
Carbonic acid	.304
	1.000

Or, removing the quarts,

Protoxide manganese	.560, or	Carb. manganese	905
Lime	.054	Carb. lime	095
Carbonic acid	.386		1.000
	1.000		

Carbonate of manganese from Freyberg gave,

Protoxide of manganese	.510, or	Carb. manganese	.822
Ditto of iron	.045	Carb. iron	.073
Lime	.050	Carb. lime	.089
Magnesia	.008	Carb. magnesia	.016
Carbonic acid	.387		
	1.000		1.000

Ann. des Min. vi., 594.

3. *Native Chromates of Iron.*—The following is the composition of two chromates of iron, analysed by M. Berthier, the first from St. Domingo, the second from Baltimore.

Peroxide of iron	.370	.350
Oxide of chromium	.360	.516
Alumine	.215	.100
Silica	.050	.030
	.995	.996

Ann. des Min. vi., 577.

4. *Hydrate of Alumina.*—A native hydrate of alumina, from Beaux, department des Bouches-du-Rhone, analysed by M. Berthier, gave,

Alumine	.520
Water	.204
Peroxide of iron	.276
Oxide of chrome, a trace	
	1.000

M. Berthier considers the oxide of iron as merely mixed with the mineral; its true constitution therefore is,

Alumine	.72
Water	.28

The mineral occurs in pieces of indeterminate form, and sometimes in round grains, agglutinated by a paste of the same nature, penetrated by carbonate of lime. The ferruginous matter is of a blood-red colour, its fracture uniform and shining, never radiated, its specific gravity small and variable.—*Jour. de Min.* vi., 531.

5. *Native Nitrate of Soda.*—An immense quantity of nitrate of soda has been discovered in Peru, in the district of Jarapaca, a short distance from the frontiers of Chili. It forms a bed many feet in thickness, which in many places appears at the surface of the soil, and occupies an extent of forty leagues. The salt is found sometimes effloresced, sometimes crystallised, but most frequently mingled with clay and sand. It is bitter, and deliquescent; it contains a little sulphate of soda.

The site of the mineral salt is about three days journey from Conception, a port of Chili, and from Iquiqui, a port of Peru.

Above 6000 quintals of the purified salt have been taken to these parts for the purpose of trade. It may be very advantageously used in the production of nitric acid.

6. *Recent Iron Pyrites.*—Professor Meinecke observed, on the Jolauer Heath near Halle, tables of an inch in breadth of iron pyrites, intermixed with reeds, and which he observed continued to increase in size; thus proving their new formation.

7. *Remarkable Glaciers, containing Organic Remains.*—Mr. Otto von Kotzebue, in his Voyage to the South Sea and Behring's Straits remarked a singular formation of ice in the western part of the gulf which he discovered to the north of Behring's Straits, and to which he gave his name.

On the 4th of August, having ascertained the longitude (161° 42′ 20″ W. of Greenwich) and latitude (66° 13′ 25″ N.) of the place where they had cast anchor, they left the Rurick, with two boats provided with several days' provisions. In the place where they passed the night, the earth rose rapidly from the flat shore to a height of 120 feet, and then appeared as an immense plain, covered with moss, a little herbage being found on the inclined surface. An English mile from the shore the depth of the sea was only five feet.

In the place where they passed the fourth night, the rise of the surface from the shore was not great, but after that it attained a considerable elevation, abundance of herbs being found at the bottom, and above, moss.

On the 8th, a tempest prevented their return to the ship, but gave occasion to the remarkable discovery made by Dr. Eschholz, whose name was given to the bay.

"We had travelled considerably in various directions, without perceiving that we were upon glaciers. The doctor having made an excursion to some distance, arrived at a ruin of the mountain, when, to his surprise, he discovered that the whole of its interior was pure ice. Immediately we all went to the spot, provided with tools. We soon came to a place where the shore rose nearly perpendicularly above 100 feet, and then continued to extend with a slight inclination upwards. Here we saw masses of the purest ice above 100 feet high, which were preserved under a carpet of moss and herbs. They must have been formed by some terrible convulsion. The portion which had fallen down being exposed to the sun and air, melted, and much water ran from it into the sea.

"An indubitable proof that the ice before us was primitive ice, is afforded by a great number of the bones and teeth of the mammoth, left when it is melted. I found one very fine tooth. We could not explain to ourselves the cause of a very strong odour, which exhaled from this country, similar to that of burnt horn.

The soil of these mountains, which, to a certain height, is covered with abundant herbage, is only half a foot thick. It is a mixture of argillaceous earth, sand and earthy matter. The ice gradually fuses below the verdure, has its position changed, and continues growing in a lower position, and in this manner one may conceive it possible that, in a long series of years, the mountain may disappear, and a verdant valley occupy its place."

By observation we found the latitude of this tongue of earth 66° 15″ 36′ N. At the bottom of the eastern part of the Bay of Eschholz, high mountains were perceived.

8. *Cave containing Rattlesnake Skeletons.*—About the year 1748, some labourers in working a quarry in the neighbourhood of Princeton, in America, for the stone with which the college is built, discovered a small cavern, which contained the entire skeletons of an immense number of the rattlesnake. The bones were in such quantity as to require two or three carts for their removal. There can be little doubt that this cavern had once a small opening, which was afterwards closed by the accidental fall of a stone, or some other impediment. This cave had probably been the winter abode of the rattlesnake for years, where many had died through age, and others in consequence of the circumstance just mentioned. Such facts as these have gained an interest since Mr. Buckland's description of the cave at Kirkdale, from the relations that are evident between them.

9. *Meteors.*—A very large brilliant meteor passed over the city of Richmond (Virginia), on the 16th March, about five minutes after ten P. M., in a direction from the north-east to the south-west. It is described as being nearly as large as a barrel, emitting sparks in every direction, and leaving behind it a large train of light; some even thought they heard a hissing noise as the meteor passed over them. The light was bright, silvery, and very powerful. It exploded with a loud noise, leaving a wide stream of fire, which was visible for several minutes.

On the 9th of April, about nine in the evening, a large meteor was seen at the town of Rodiz in Avignon. It appeared like a pillar of fire, sending out an infinite number of sparks. After some seconds it disappeared, and a loud explosion was heard.—*Phil. Mag.* lix, p. 399.

10. *Analysis of the Meteorolite of Juvenas.*—A meteoric stone fell at Juvenas on the 15th of June, 1831, of which a short notice was given in a former number of this Journal. An aualysis of it has been made by M. Laugier, who finds it to accord with that from Jonzac, in containing no nickel, and in containing chromium. He therefore urges it as a further proof of the propriety of considering chromium as the distinctive metal of aërolites, at least till one or more of them has been found without it.

The stone from Juvenas resembles other meteoric stones in its aspect, but is more friable than most of them. It contains no globules of iron sensible to the pestle, but small crystals may be seen in it, having an evident cleavage, which have received the name of feldspar, and appear by analysis to be that substance. Acids only acted on it partially, but after fusion with potash, its elements, by further treatment, readily separated from each other. They are given as follows:

Silica	40.
Oxide of iron	23.5
Oxide of manganese . .	6.5
Alumine	10.4
Lime	9.2
Chromium	1.0
Magnesia	0.8
Sulphur	0.5
Potash	0.2
Copper	0.1
Loss from unknown causes	4.8
—— indispensable . . .	3.0
	100.0

The circumstance of a constant loss, rather than an increase of weight, induces M. Laugier to conclude that the iron and magnesia are in the state of oxides in the stone. When powdered, no part of it was attracted by the magnet. The loss, however, remains unaccounted for. When a portion of the stone was distilled it lost in weight, but M. Laugier could not satisfy himself of the substance gone off; it was neither water nor carbonic acid gas.

The analogy of this aërolite to that of Jonsac is remarkable: the entire absence of nickel, the disappearance almost complete of the sulphur and magnesia, with the presence of an abundant quantity of lime and alumine, establish a considerable difference between these two stones and those previously known to have fallen from the air.

Reference is then made to several other aërolites which fell the 13th of December, 1813, in the neighbourhood of Lantela, in the government of Wibourg in Finland. Mr. Nordenskiold, who examined them, describes them as very friable, containing grains of olivine, and also amphigene, scarcely at all attracted by the magnet, and containing no nickel.

Considering the difference in aspect and composition between these three stones and those which were previously known, M. Laugier thinks that these substances may be arranged in two classes, but he does not insist much on this division.—*Annales de Chim.* xix. 264. *(See our Foreign Science.)*

11. *Volcanoes in Iceland.*—Violent volcanic eruptions took place last winter in Iceland, where they were wholly unlooked for. The mountain called Oefields-Jokkeler, to the south-east of Hecla, which had been quiet ever since the year 1612, broke out with great fury on the 19th, 20th, and 21st of December, 1821, so that the ice, with which it was covered, burst with a tremendous crash. The earth trembled, and enormous masses of snow were precipitated from the summit (a height of 5,500 feet) into the plain. From that time, a column of fire continued to rise from the crater, which ejected vast quantities of ashes and stones, some of them weighing from 50 to 80 lbs., half calcined, were thrown to the distance of five English miles from the crater. It does not appear that any great damage has been done by this eruption; the mass of sulphureous ashes which covered the adjoining country like a thick crust, has since been removed by a violent storm and torrents of rain. The mountain continued to burn till the 1st of February, and smoked till the 23d, but the ice had again formed round the crater. During the eruption, the weather in the island was extremely unsettled and stormy, with loud noises and sensible shocks as of an earthquake.—*New Monthly Mag.*, vi. 217.

12. *Volcanoes in the North.*—Two volcanoes have broken out in the North, at the eastern boundary of Asiatic Russia, viz., at the western extremity of North America. The inhabitants of the Aleutes Isles (or Foxes) perceived, in the night of the 2d of March, 1821, all the signs of a great natural commotion. A strong S.W. wind, violent shakings of the earth, and subterraneous noises; immediately after the atmosphere was inflamed, and sand, with cinders, fell during the whole night. While this was passing at Unalachka, another volcano burst forth at Ounimach, an island about 24 leagues from Unalachka. Columns of smoke and fire rose from the latter place until the month of August. The land has been much altered by these commotions, the sea having receded to a considerable distance.

§ 2. Medicine, &c.

13. *Proper State of Prussic Acid for Medicinal Use.*—A series of experiments have been undertaken by a company of associated physicians, surgeons, and naturalists at Florence, to determine the best state of the hydro-cyanic or prussic acid for medicinal purposes. The experiments were made with great care, and varied several ways. Different preparations of the substance were used, rabbits being the animals on which they were tried. Their joint opinion is expressed as follows: "We may then conclude, from our researches, that the essential oil of the prunus lauracerasus is to be preferred in medical practice to all other prepara-

tions which contain the hydrocyanic acid; for, unlike the distilled water of the plant and pure prussic acid, it contains the same proportion of the acid, and is of the same power, whether recently prepared or old, when made in one place or another, after exposure to the air, to light, or to heat. We think also that the oil of olives, or of almonds, is the most proper vehicle in the proportion of an ounce to 12 drops of the essence, or in a smaller dose when employed by friction externally."

14. *Oil of Turpentine rendered palatable.*—Dr. Nimmo employs the following method to purify the oil of turpentine for medicinal use, "without diminishing its efficacy, but greatly lessening its disagreeable taste, and its injurious effects upon the kidneys." To 8 parts of the oil add 1 part of the strongest alcohol, and let them be well agitated. In a few minutes a separation takes place, the oil, unless very impure, falls to the bottom, and the alcohol having discharged the impurities, floats at the top. Pour off the alcoholic portion, add an equal quantity of the alcohol as before, agitate and separate the liquids. If this be repeated three or four times the oil becomes nearly tasteless, almost without smell, and when a portion of it is evaporated, it leaves no residuum. It is necessary to remark, that pure as the oil may be rendered, it speedily undergoes alteration, and returns to its original state of greater or less impurity. *(See Dr. Nimmo's Paper, in our last Number.)*

15. *Hooping-Cough.*—Dr. Archer, an American physician, announces that the hooping-cough is cured by vaccinating the patient on the second or third week after the commencement of the disease. This is a singular discovery, and if the result be doubted, the experiment is at least harmless.

16. *Large Human Calculus.*—A large human calculus has been described by Professor Cumming, of Cambridge: it weighs 32 ounces, and measures 15½ inches in circumference. Its specific gravity is 1.756. The nucleus is lithic acid, and to this succeeds a considerable portion of the oxalate of lime, then layers of the triple phosphate, covered by a thick coating of lithic acid, the external surface being composed principally of the fusible calculus. It is in the possession of Trinity College. A calculus is also noticed from the intestines of a hare: It is composed of vegetable matter and the phosphates.

17. *Physiological Prize Question.* The Royal Academy of Sciences have proposed, as a prize question for 1823. "To determine by precise experiments, the causes, either chemical or physiological, of animal heat." It is required that the heat emitted by a healthy animal in a given time, and the quantity of car-

bonic acid produced, be exactly determined; and that the heat be compared to that produced by the combustion of carbon, whilst forming the same quantity of carbonic acid. The prize is a gold medal of 3000 francs value. Memoirs to be sent in before Jan. 1, 1823.

18. *Prize Questions, College of Surgeons.*

Jacksonian Prize.—Not either of the prizes for 1820 having been adjudicated, three prize subjects are proposed for the year 1822; viz. Injuries and Diseases of Muscle, Diseases of the Skin, and the Diseases of the Rectum.

Candidates are to be members of the College, and the dissertations are to be written in English, and the number and importance of facts will be considered principal points of excellence. Each dissertation is to be distinguished by a motto or device, and accompanied by the name of the person, sealed up, but distinguished by the same motto. They are to be sent to the secretary before Christmas-day, 1822.

Art. XIX.—METEOROLOGICAL DIARY for the Months of March, April, and May, 1822, kept at Earl Spencer's Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern spect, about five feet from the ground, and a foot from the wall.

For March, 1822.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Friday - - - -	1	24	44	30.12	30.04	SW	SW
Saturday - -	2	40	50	30.04	30.08	NW	SW
Sunday - - -	3	42	52	30.10	30.04	SSW	SSW
Monday - - -	4	40	52.5	29.94	29.78	SE	SSW
Tuesday - -	5	42	52	29.78	29.61	WbS	W
Wednesday -	6	45	53	29.30	29.37	SW	SW
Thursday - -	7	37	50	29.36	29.30	W	W
Friday - - - -	8	32	46	29.47	29.20	W	W
Saturday - -	9	37	51	29.42	29.50	W	WbS
Sunday - - -	10	47	55	29.38	29.36	WbS	W
Monday - - -	11	36	48	29.52	29.81	W	W
Tuesday - - -	12	30	50	30.14	30.18	WbN	S
Wednesday -	13	33	49	30.07	29.83	S	SbE
Thursday - -	14	40	53	29.72	29.87	SW	WbS
Friday - - - -	15	27	52	30.03	30.03	WbS	S
Saturday - -	16	32	52	30.00	29.90	SE	SE
Sunday - - -	17	43	50	30.06	29.90	SW	WbS
Monday - - -	18	44	51	29.98	30.20	NW	WbS
Tuesday - - -	19	44	57	30.06	30.10	WbS	W
Wednesday -	20	49	56	30.10	30.10	W	W
Thursday - -	21	42	58	30.10	29.97	W	W
Friday - - - -	22	39	53	30 00	30.18	WbS	WbS
Saturday - -	23	35	57	29.99	29.81	SW	WbS
Sunday - - -	24	42	50	29.45	29.49	WNW	SW
Monday - - -	25	33.5	48	29.51	29.53	SW	W
Tuesday - - -	26	35	55	29.80	29 87	W	W
Wednesday -	27	43	54	30.00	29.93	SW	SW
Thursday - -	28	43	64	29.79	29.79	WbS	W
Friday - - - -	29	40	58	30.10	30.20	W	SW
Saturday - -	30	44	50	29.60	29.31	WbS	NW
Sunday - - -	31	34.5	43	30.21	30.28	NW	NNW

For April, 1822.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Monday - - -	1	35	48	30.27	30.21	N	NW
Tuesday - - -	2	38	47	30.21	30.21	NW	NNE
Wednesday -	3	39	48	30.25	30.19	NW	WbN
Thursday - -	4	38	50	30.10	30.04	NW	NW
Friday - - - -	5	39	53	29.95	29.87	WbN	NW
Saturday - -	6	38	51	29.79	29.73	WbN	NW
Sunday - - -	7	36	50	29.86	29 85	N	ENE
Monday - - -	8	32.5	50.5	29.90	29.90	NE	NE
Tuesday - - -	9	36	46	29.90	29.96	NbE	NE
Wednesday -	10	315.	44	29.99	29.94	NE	ENE
Thursday - -	11	31	43.5	29.94	29.91	E	E
Friday - - -	12	36	44	29.67	29.60	ENE	E
Saturday - -	13	38	54	29.60	29.70	ESE	SSW
Sunday - - -	14	41	59	29.84	29.84	SW	S
Monday - - -	15	46	56	29.84	29.84	S	EbS
Tuesday - - -	16	47	57	29.84	29.84	NNE	NW
Wednesday -	17	40	54	29.80	29.68	SSW	NW
Thursday - -	18	40	53	29.59	29.51	W	NW
Friday - - - -	19	41	60	29.54	29.57	W	NW
Saturday - -	20	44	61	29.53	29.50	WbS	SSW
Sunday - - -	21	42	58	29.44	29.30	S	SE
Monday - - -	22	44	55	29.22	29.20	SW	SE
Tuesday - - -	23	37	52	29.18	29.26	E	WSW
Wednesday -	24	38	55	29.41	29.43	SE	SE
Thursday - -	25	44	54	29.35	29.41	SSW	SW
Friday - -	26	39	58	29.57	29.80	SW	WSW
Saturday	27	43	56	29.80	29.83	SE	NW
Sunday - - -	28	36	61	30.08	30.08	ESE	SE
Monday - - -	29	41	65	30.10	30.08	SE	SE
Tuesday - - -	30	46	64	30.11	30.11	NE	E

For May, 1822.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Wednesday -	1	41	65	30.19	30.18	E	E
Thursday - -	2	34	65.5	30.10	29.96	NE	SE
Friday - - - -	3	44	63.5	29.89	29.70	ESE	SE
Saturday - -	4	48	63	29.50	29.50	E	E
Sunday - - -	5	45	60	29.56	29.69	ENE	NE
Monday - - -	6	49	64	29.60	29.60	EbN	NE
Tuesday - - -	7	50	55	29.60	29.70	NE	EbN
Wednesday -	8	43	52	29.81	29.84	NNE	NE
Thursday - -	9	33	50	29.69	29 49	E	NE
Friday - - - -	10	39	48	29.36	29.15	NE	SE
Saturday - -	11	38	61	29.40	29.53	ENE	E
Sunday - - -	12	45	50	29.73	29.73	NE	NE
Monday - - -	13	43	56	29.77	29.75	NE	NE
Tuesday - - -	14	40	62	29.77	29.73	NW	WbN
Wednesday -	15	42	71.5	29.73	29.76	W	W
Thursday - -	16	41.5	65	29 84	29.81	E	E
Friday - - - -	17	41	71.5	29.82	29.82	NE	EbN
Saturday - -	18	53	70	29.85	29.88	NW	NW
Sunday - - -	19	47	71	29.95	29.91	NW	E
Monday - - -	20	48	72.5	29.96	30.00	E	NW
Tuesday - - -	21	46	73	30.10	30.13	NE	NE
Wednesday -	22	49	64	30.20	30.18	NE	ENE
Thursday - -	23	45	61	30.17	30 08	ENE	NE
Friday - - - -	24	47	64	29.98	29.92	NE	ENE
Saturday - -	25	43	68	29.89	29.76	NE	ESE
Sunday - - -	26	41	62	29.63	29.70	SE	WbS
Monday - - -	27	41	67	29.90	29.90	SW	WSW
Tuesday - - -	28	55	69	29.97	30.06	WbS	N
Wednesday -	29	46	71	30.08	30.08	N	WbN
Thursday - -	30	41	67	30.17	30.10	W	W
Friday - - -	31	48	72	30.09	30 10	WSW	W

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